What Language is Made From: Helen Keller, Some UG Residues, and the Strong Minimalist Thesis

Norio SUZUKI

Abstract

Reconsidering and reanalyzing a number of problems with first language acquisition and learnability within the framework of the Minimalist Program, the most recent linguistic theorizing on the part of generative grammar, the present paper ascertains some theoretical implications and consequences of the syntactic/learnability framework comprising Suzuki (2001, 2002, 2003, 2005, 2006) for the purposes of explicating the long-standing problems: (i) the transition from "pidgins" to "creoles"; (ii) the emergence of a Nicaraguan sign language among deaf children who had no common communication means (presumably, involving a generational stage where something like the transition from "pidgins" to "creoles" may be observed); (iii) the emergence of what are called "home signs," which are developed by deaf children who are not exposed to a signed language during the major part of the critical period; (iv) what the languages of specific language impairment (SLI) patients are like; (v) what the languages of wolf children such as Genie are like; and (vi) the outstanding case of Helen Keller, where a "tactile sign language" has been reported to have been involved (see Gill 1997). (Although I believe that resources present in the framework of Suzuki 2005, 2006 in particular possess the potential enough to cope with most major problems arising in the explanation of the questions in (i-v) above, I specifically take up in the present article the fifth question of what Helen Keller's language is like, submitting the more or less in-depth scrutiny and explanatory treatment (apart from some sporadic mentions of them during the course of theoretical developments appearing in the present paper) of the other five questions to (my) future work.) In view of its potential for locating the possible origins of important differences between the cases of the Japanese second language learner of English, for example, and of Genie, in both of which the learner seems to be forced to start her acquisition of her target language largely after the critical/sensitive period has passed, the present paper focuses on the fascinating feat of language acquisition on the part of Helen Keller, who was deaf, blind, and mute as a result of the high fever and severe stomachache that hit her when she was one year and nine months, the most interesting point of Keller's outstanding case being that Keller somehow appears to be located between the (normally developing) second language learner and wolf children such as Genie in terms

of her linguistic abilities. The central concept here is Chomsky's (2000, 2001, 2004, 2006) strong minimalist thesis (SMT), possible requirements on the part of "efficient computation" and of the "evolution of language" being also considered. More specifically, the biolinguistic idea following from Chomsky (2004) should be that the initial state of language acquisition is not fully genetically-determined, but that it is also a function of the general property of organic systems and, more generally, it reflects the properties of the physical world. Further starting with some important similarities between the Unconscious (in the sense of Freud) and FLN/language and focusing on the term "compression" as a mental/linguistic vehicle between different dimensions (see Nakazawa 2004, Moro 2004), the present paper gives conjectures on the intriguing concept of a "dimension" and examines some empirical domains where the concept applies (see Uriagereka and Pietroski 2002, Uriagereka 1999/2002). And, rather importantly, some efforts are made (by following Chomsky 2006 in reducing the contents of FLN/UG to the minimum) to examine in some detail what I take to be two major UG residues, Case and the EPP, so as to attain "ontological adequacy" in the sense that the major current concern is to explain why languages are the way they are.

Keywords: strong minimalist thesis, Helen Keller, learnability, signed language, FLN/UG, evolution, UG residues, modality-independence, supergrammar, top-down approach to syntactic derivation, dimensions

1 Introduction

The present paper is part of the larger ongoing project that mainly aims to explore the theoretical implications and consequences of Chomsky's (2000, 2001, 2004) strong minimalist thesis (SMT) in explaining the problem of learnability (the logical problem of language acquisition; among other things) as it applies not only to connection with such familiar interfacing modules as PF, LF, and pragmatics (see López 2003), but also to other empirical domains such as acquisition, processing, neurology, language change, and so on (see Chomsky 2000). With special reference to the current concern over the "evolution of language" and the long-standing desirability of efficient computation, it also discusses the nature of interfacing, the right concept of economy, and the true nature of variation across languages.

Somewhat anticipating the main concluding directions that some of the topics that can safely be said to come within the explanatory potential of the paper will take, I here briefly touch on them on the basis of some of the major assumptions that constitute the backbones of the framework adopted (see Suzuki 2001, 2002, 2003, 2005, 2006). As for the language of specific language impairment (SLI) patients, I claim that while biolinguistic resources are responsible for the unimpaired portion of their language, they may have to resort to what is called the "general learning strategy (GLS)" for the purposes of dealing with the impaired part. Under the assumption that there may not be as much difference

between first and second language acquisition as has standardly been believed (as has been argued in Suzuki 2001), the FLN must be still operative in the second language learner (in the post-critical period) while it is not in Genie (after she was rescued in her post-critical period). According to Suzuki (2001), the FLN is biolinguistically operative in the post-critical period second language learner because of her FLN (biased in the direction of her target language) being in a working state for the purposes of everyday performances, whereas in Genie it must be in a (biolinguistically or neurophysiologically) "frozen" state just because it has never been biolinguistically developed or used for lack of stimuli in the critical/sensitive period, resulting in the widely observed case of the fairly poor linguistic performance on the part of Genie in spite of many psycholinguists' efforts to teach her language. And I assume that what little language she learned must only have been obtained through GLS. As for Helen Keller's language (specifically, what she obtained/-learned/acquired when she "became a member of the speaking community" as a result of the remarkable event at the pumphouse; see Gill 1997), the relevant concept should be the modality-independence of the FLN; that is, it should in principle be available in enabling language to be realized in any way/form, spoken, signed, tactile (Helen Keller, Braille), or whatever. And to the best of my belief, the much less discussed fact that Helen Keller had been growing up as a "normally developing child" until she fell sick at the age of one year and nine months with a high fever and stomachache for which there was no obvious cause, ending up as one who was deaf, blind, and mute (the last property being a byproduct of the first) should also be included among the major considerations indispensable for the clarification of her fascinating case.

2 Some General Discussion from the Perspective of the Framework Adopted So Far

The set of important assumptions appearing in the present paper include the following: (i) interfacing components are "invasive" (see Epstein et al. 1998, López 2003, Suzuki 2005), so that interfacing components/systems themselves can have access to the narrow syntactic derivation roughly to assign/attach linguistic features in some way to the syntactic structure under derivation at the time of spell-out/TRANSFER; (ii) the concept of TRANSFER in the sense of Chomsky (2004) is strengthened (the "strengthened mapping hypothesis SMH" of Suzuki 2005, 2006), so that bi-directional passage can be established between narrow syntax and interfacing components/systems at the completion of each strong phase (CP, vP) with application of the strengthened TRANSFER operation subject to economy in the sense of Fox (2000), inviting further possibilities of (bi-directional) passage being established between interfacing components/systems *via narrow syntax* (NS); (iii) the "lexical condition on language acquisition after birth" (see Suzuki 2006), such that the child's language acquisition *after birth* is limited to the acquisition of the lexicon/lexical items of her target language, which may lead to some viable explanation,

among other things, of the pidgin-creole connection/transition without resorting to an overly strong innateness hypothesis such as Bickerton's (1999) Language Bioprogram Hypothesis (LBH), which would have hard time accounting for variation across languages, of the famous case of the naturalistic emergence of a common signed language among Nicaraguan deaf children who had been brought up at completely different places with a quite different signed language, perhaps with the actual situation having been a near replication of the hearing people's pidgin-creole connection/transition, and of the best-known but quite recalcitrant case of Helen Keller; (iv) the concept of *supergrammar* SG of Fodor (1998), such that the human being continues all her life to possess the faculty of language in the narrow sense (FLN; see Hauser, Chomsky, and Fitch 2002; putting aside the very important problem of the lexicon for the time being), i.e., roughly, the invariant principles and parameters defining variation, with the parameter options selected during the critical/sensitive period having a special status for her target language, yet the other *now unselected* options continuing to exist and be available at any time of her life, which picture may readily open the way to the plausible account of the actual situation with language where languages are much more unstable and subject to change (see Lightfoot 2006; an example from a play by Shakespeare: "Speak not. Do not say anything," uttered by one speaker at one time, where we can see at once both options of the "V-to-T parameter"), and obviously be compatible and congenial with Yang's (2002) theory of acquisition (and change) in terms of the biological theory of population genetics; and (v) the top-down approach to syntactic derivation (see Suzuki 2006; also Phillips 1996, 2003, Terada 1999, 2002), which somehow seems to be forced, given SG. (However, at the time I decided in Suzuki 2006 to adopt some species of top-down approach to narrow syntactic derivation, I somehow did not take into consideration the important set of data from language pathology pertaining to the determination of how sentences are built up. Some agrammatic aphasic patients with a deficit that usually occurs following brain lesion in Broca's area and its vicinity in the left hemisphere, for example, have been reported to lack CP-structures when they have problems with tense, that is, ones at the level of TP (see Friedmann 2006). So in the present paper I take up the validity of the adoption of a top-down approach to syntactic derivation from this new perspective, and specifically reconsider the ontological status of the true nature of narrow syntactic derivation.)

That is, the situation with respect to derivational computation should be roughly as follows, according to the scenario of the present paper (limiting the discussion now to spoken language): Suppose that Chung Yoo Jin is an adult woman speaking Korean. FLN (consisting of narrow syntax (NS), the semantic component (Σ), and the phonological component (Φ); that is, containing the property of recursion and the mapping mechanisms to the interfaces in the terms of Fitch, Hauser, and Chomsky 2005) in her brain somehow has all the UG parameters biased toward Korean (thanks to parameter-setting during the critical/sensitive period), all else being common and the same across all speakers of all languages (here again, abstracting away from the important presence of the lexicon of her

target language, which I provisionally assume to interface with FLN). I assume that the Korean-biased FLN in Yoo Jin's brain is (always) available for use, presumably through accessing from outside. What accesses FLN? I assume that it must be the parser/processor ("interpretation-wise") and the producer (my term; "production-wise") that access FLN and, moreover, access the lexicon interfacing FLN via FLN, under the assumption that these performance modules interact/connect/interface with FLN. They access relevant parts/portions of FLN: e.g., the (strongly biased) OV option of the head-parameter, but not the VO option (which may be available in any case, given SG); the operation Merge in order to make use of it for derivational computation, along with the property of recursion; various lexical items through access to the lexicon via FLN/NS. The standard assumption has been that the parser/processor and the producer are performance modules. This ends up bringing us to assumption (v) above, which seems to force us to adopt the top-down approach to syntactic derivation rather than the long-practiced, standard bottom-up approach to it, simply because performance in general proceeds in a time-bound, top-down (from sentence-initial to final position) fashion (and I also turn to Friedmann (2006), where agrammatic aphasic patients do not have for the purposes of sentence production structures higher than the hierarchical level at which they have a problem). It may follow from the assumptions so far that all that FLN does must be to provide raw materials to be accessed for *grammar* formation in the form of invariant principles and parameters responsible for variation (perhaps, with some set of parameter options strongly biased toward Yoo Jin's first language and the other set simply neutral) and that a *grammar* only appears/emerges in linguistic performance such as parsing/processing and production in a strictly time-bound fashion, leading to the idea that it is raw materials provided by FLN (and other components) that are *physically/biologically real*, while grammars are simply epiphenomena arising from performance in a time-bound fashion. Notice that this approach to syntactic computation may give us an interesting answer to the long-standing problem of intentionality with Chomsky's system that has been bothering many of his critics (see Rey 2003) once we commit ourselves to the claim that performance modules responsible for the production of derivations/representations may somehow function in some *intentional* (which concept I hope will be clarified below) manner. Then it should by now be clear that the system above calls into question the empirical, factual basis on which to establish the validity of the long-practiced, traditional bottom-up approach to syntactic derivation (with possible reservations arising from some neuro-anatomical studies specifically pertaining to Broca's area; see above).

Among the related topics in addition to those mentioned above that I intent to touch on in the paper are included the alternative explanation of the processing cost involved in the acquisition of stress shift and focus (see Reinhart 2004, 2006), the long-standing problem of how to view the notion of "intentionality" in the general framework of Chomskian generative linguistics, and some conjectures on Case and the EPP as two major residues of UG/FLN in the context of most recent biolinguistic theorizing with the SMT as the

central guiding concept (see Chomsky 2006).

2.1 On the Residues of FLN/UG: Case and the EPP

Chomsky (2006) is an interesting attempt to view UG (interpreted as the theory of the initial state of FLN) "from below," the overall picture being that given the SMT (which, roughly, requires FLN/UG/language to meet interface conditions deriving largely from relevant external systems such as sensorimortor (SM) and conceptual-intentional (CI) ones, with interfaces such as PHON and SEM derived through FLN operations, primarily consisting of narrow syntactic ones (with the sole operation of Merge and the sole property of recursion, *and Case and the EPP*, the two major residues of FLN I look into in this section) and operations belonging under each relevant component (such as the phonological component (Φ) and the semantic component (Σ)) connecting with a relevant interface) and the ontology of FLN, the objective of much recent minimalist work has been to close the gap between the two, and that *FLN/UG is what remains when the gap has been reduced to the minimum and also when all third factor effects* (of the sort that enter into all facets of growth and evolution, including "efficient computation," among others) *have been identified.* Chomsky (2006: 3) further assumes that *FLN/UG mechanisms/residues had arisen in the course of evolution of language.*

According to Chomsky (2006: 6), the property of unbounded Merge (presumably, due to the property of recursion) reduces to the statement that lexical items (LIs) have an edge feature (EF), since under the no-tampering condition (NTC) Merge will always be to the edge of a lexical item or a constructed structure. Chomsky (2006), moreover, observes that the only syntactic properties of UG are that it contains Merge and LIs with undeletable EF (with EF undeletable as a result of a UG property), and that expressions generated must satisfy interface conditions (i.e., SMT) in a principled way (i.e., with efficient computation, a third factor condition, operative). However, apart from Merge, recursion, and the mapping to the interfaces (see also Hauser, Chomsky, and Fitch 2002; Fitch, Hauser, and Chomsky 2005: 182), I would like to focus on what I believe to be two major residues of FLN/UG, that is, two instances of *what remains when the gap* (between the SMT and the ontology of FLN) *has been reduced to the minimum and when all third factor effects have been identified:* (structural) Case and the EPP, and see to what extent these concepts/entities may be derived from other, hopefully non-linguistic or at least FLN-external properties, or if this option is impossible, whether they can be reduced to other viable FLN notions.

2.1.1 Case

As for structural Case, Case valuation is implemented in a manner of an ancillary sort under Agree, the latter predominant operation presumably being able to be taken to be based on the presence of some fragments of the *reality of the world* as is evident in what agreement features derive from (e.g., "third person, singular, feminine" corresponding to "those other than speaker and hearer, (only) one, a woman/girl," respectively). So, keeping to nom-acc languages, I take what may be involved in the checking of the subject, for example, in terms of Agree/Case (with structural Case as a reflex of an uninterpretable ϕ-set; see Bošković and Lasnik 2007: 79: from Chomsky 2000a) to roughly be: (i) the primary ϕ-valuation of T-probe (which may immediately be deleted due to its uninterpretability) on the basis of the ϕ-values of the subject-goal (which should still be downstairs, thanks to the assumption adopted here that Agree/Case and the EPP are divorced) and (ii) the secondary, concomitant Case-valuation of the subject-goal on the part of T-probe (in the form of nominative Case, in this case; with the Case of the subject-goal immediately deleted, due to the inherent uninterpretability of Case, by hypothesis, which theoretical status of Case I believe should be attributed to its status *as an FLN residue*, with no place other than FLN/UG for the purpose of its existence, and, certainly, with every possible external interpretation relevant to the present purposes of deriving grammatical devices only available in domains other than FLN/UG).

Alexiadou and Anagnostopoulou (2001) is an extremely interesting attempt to spell out the intriguing nature of the role of (structural) Case in narrow syntactic derivation, which should be independent of the EPP, another concern here as an FLN/UG residue (see below). Their (2001: 193) central generalization is as follows:

(1) By Spell-Out VP can contain no more than one argument with an unchecked Case feature.

The theoretical framework of Alexiadou and Anagnostopoulou (2001) is primarily based on Chomsky (1995b: chapter 4; 2000a). Since the present paper is couched in a more recent version of minimalism, I point out necessary adjustments for the purposes of more recent concerns (when needed) as I proceed with my discussion on what Alexiadou and Anagnostopoulou (2001) have to say in regard to Case, a quite intriguing UG residue that should play a major role in explaining the true nature of FLN/UG, which is assumed to be the exclusive possession of *Homo sapiens*. While Alexiadou and Anagnostopoulou (2001) seem to point out a numer of interesting phenomena crucially involving the notion of (structural) Case, the present paper departs from them (2001: 226), where "the EPP and Case both force argument externalization," in assuming that it is only the EPP that is concerned with the (non-)realization/(un)availability of syntactic positions in the sentence. I would, however, like to take up some Japanese examples discussed in Alexiadou and

Anagnostopoulou (2001: 203) like the following, though in a framework with somewhat different assumptions, as noted:

(2) a. John-ga/no nihon-e kaetta hi
John-NOM/GEN Japan-to returned day
'the day on which John came back to Japan'
b. John-ga/*no *LGB*-o kashita hito
John-NOM/GEN *LGB*-ACC lent person
'the person to whom John lent LGB'
c. [John-ga/no t katta] hon
John-NOM/GEN bought book
'the book John bought'

In (2c) 't' is the trace/copy either of the relative operator associated with the head hon or of the nominal element hon itself, depending on the analysis adopted of the restrictive relative construction. While it may be possible to account for the facts in (2) in terms of the generalization in (1), which regulates the number of arguments with an unchecked (structural) Case feature in VP/vP at a certain derivational stage (see Alexiadou and Anagnostopoulou 2001: 203 for further details), I explore another possibility of accounting for the Japanese examples in (2), due to the serious difference noted above between their (2001) position and mine in the treatment/interpretation of the EPP and Case. Sigurđsson (2006a) proposes an extremely interesting principle called the *low nominative hypothesis*, where the central concept is the *Sibling condition*, which states that accusative Case enters the derivation later than nominative, the presence of the former somehow being dependent on that of the latter. Keeping to the *-ga* and *-o* Cases (i.e., nominative and accusative in Japanese) in the exposition of (2), there is no problem with (2a) or (2c), where an argument with -o (accusative) Case is simply missing (I return to (2c) below), in connection with the Sibling condition, which is only concerned with the specific derivational relationship between nominative and accusative. The reason that the version of (2b) with the subject with *-no* (genitive) Case is bad is staightforwardly clear. It is bad because an *-o* Cased-marked argument appears in (2b) *without a -ga Case-marked one*, in violation of the Sibling condition, which requires nominative Case to be Merged first in the derivation, crucially earlier than accusative, and limits the presence of accusative to the cases where a nominative Case-marked argument can be found. Let us then take up some problems with morphological Case, in connection with abstract Case. Look at the example in (3b) (taken from a newspaper) along with some more retated to it in a certain way:

(3) a. Iraq-ga America-o yaburu.
Iraq-NOM America-ACC defeat
'Iraq defeated America'

a'. America-o [Iraq-ga (America-o) yaburu]. (with the object scrambled and its unpronounced copy enclosed in parentheses)

b. Iraq America yaburu. (without "particle" Case-markers)
'Iraq defeated America'

b'. America [Iraq (America) yaburu]. (with the object scrambled and its unpronounced copy enclosed in parentheses; interpreted differently than "Iraq defeated America")

Although only the example in (3b) is taken from a newspaper, both a) and b) versions of the example in (3) with the meaning "Iraq defeated America" can be taken to be expressions of the sort often employed for newpsper headings. The verb yaburu may well be in its infinitival form, with the verbal form itself permitted here as part of a newspaper heading (a special register) and the Tense anchoring in these examples presumably provided by the already completed fact reported in the paper that Iraq defeated America. The intriguing point here in regard to the possibility of context anchoring is that while the absence in narrow syntactic structure of such notions as time (encoded in a Tense feature) and definiteness/specificity (encoded in D) may be remedied by resorting to context resources, as noted (see also Avrutin 2006 for interesting cases in connection with some Broca's aphasics where the deficits in and absence of some functional categories/features can be remedied in terms of context resources, the observed remedying cases being limited to a small set of special registers, though), such (context) remedying resources may never be available to cases involving Case as a key factor.

I assume that both morphological and abstract Casees are UG residues *par excellence,* without any helping hand from outside of FLN/UG in their absence. That would mean that they have to manage with what they themselves possess in passing through various derivational stages, it perhaps being possible for morphological Case to resort to its *distinct* phonological/phonetic form in addition to the position(s) it occupies at some stage(s) in the derivation, and for abstract Case to resort only to such derivational positions. Consider a generalization concerning Case, as follows:

(4) In determining Case properties (i.e., nominative or accusative, for the purposes at hand), Case-*positions* are predominant as long as they are available for *narrow syntactic* derivation. Morphological Case (including the Japanese "particle" Cases) comes into play as soon as *positional* Case (i.e., Case-positions) turns unavailable for the purposes of narrow syntactic derivation.

The standard assumption concerning TRANSFER in the sense of Chomsky (2004) has been

that nominal arguments to be TRANSFERred, for example, must already be assigned at least a thematic role and (structural) Case, both of which are assumed to be assigned/-checked in virtue of the specific syntactic position involved) before being TRANSFERred to the FLN-internal components. The crucial difference between morphological and abstract Case should be that while the former may play a role after TRANSFER in positions other than Case-positions (due to its phonological visibility independent of syntactic positions), the latter cannot, precisely because the pronounced token of the Case-marked argument must have priority over its silent token (thanks to the natural assumption that in terms of sound, whose presence should provide the easiest way to express things in the specific case of spoken language, the pronounced ought to prevail over the silent, given the physical architecture of this world), and hence abstract Case cannot play its role in positions other than Case-positions for lack of its phonological visibility responsible for signaling the distinction between nominative and accusative. That is, crucially, abstract Case "with sound" has priority over abstract Case "without sound" and hence, in case that abstract Case "with sound" is not in a Case-position, it fails to signal its Case identity *in spite of the presence of its silent counterpart in a Case-position.* The example in (3b') should be a case in point:

(5) [$_{TP}$ America [$_{TP}$ Iraq [$_{vP}$ (Iraq)[(America) [(America) (yaburu)]](v+yaburu)]
(scrambled) <Case> $<\theta>$ <Case> $<\theta>$
T+v+yaburu]] (=(3b'))

I assume in (5) that Japanese is a V-to-T raising language and follow Chomsky (2005b) in accusative Case-checking/valuation, and I indicate the scrambled, Case-, and θ-postions in (5). The pronounced part consists of three portions not enclosed in parentheses. And irrelevantly, the VP portion "[(America) [(America) (yaburu)]]" is subject to TRANSFER at the completion of the strong vP phase. Reall that the two arguments *Iraq* and *America* are (morphologically) Caseless. There appears to be no problem with the argument in [Spec, T] (i.e., *Iraq*, which is morphologically Caseless) since it resides properly in a Case-position, apart from the observation that the whole sentence may tend to have an interpretation different from the intended one, on which "Iraq defeated America." That is, a morphologically Caseless argument has to be in a structural Case-position in order to continue to be able to play a role in the subsequent structure building. The problematic aspect of (5) is, of course, found in the scrambled phrase *America*, which is morphologically Caselss, thus being unable to signal its Case status by itself, *and* is not in a Case-position, leading to the conclusion that there is simply no FLN/UG way (either positionally or morphologically) to know whether the scrambled phrase is nominative or accusative Case-marked. Unlike the cases of tense and specificity, which can count as discourse notions and hence, for which there exist discourse/context resources to remedy unwanted situations (see above), *there simply do not exist any such non-FLN/UG saving resources for*

the concept of Case, precisely because Case is an FLN/UG notion with no other place for it to play a role. You may apply to (3b'/5) something like the usual "SOV" frame (for Japanese) of an obscure sort (which may be a tool belonging to the so-called general learning strategy (GLS), but definitely not a biological device) and obtain the interpretation like "America defeated Iraq," which is not the intended interpretation, of course. It seems that we are here faced with the hard reality of the physical world surrounding us. It ought to be the physically visible that wins out. The invisible with right properties cannot help its incomplete visible counterpart with its task of successfully providing the right interpretation. Let us close the section by considering the Japanese example in (2c) above from Alexiadou and Anagnostopoulou (2001):

(6) [John-ga/no (*Op* /hon(-o)) katta] hon (= (2c))

I have added the unpronounced contents of the object of *katta* (i.e., *bought*), both in terms of the *wh*-movement analysis of the restrictive relative construction (see Chomsky 1977b) and of its head-raising analysis (see Kayne 1994). The major principle regulating the appearance of differently Case-marked arguments in the structure is Sigurđsson's (2006a) Sibling condition, which bars accusative from entering the derivation in the absence of nominative. A possible problem would involve the genitive Cased-marked subject *John-no,* since the configuration would violate the Sibling condition *if the object (specifically, hon-o) were pronounced.* This situation with the differences in status (in terms of their (in)ability to take part in sentence building) between the physically/auditorily visible/audible and the invisible/inaudible may point to the major significance of the "third factor" in the sense of Chomsky (2006: 2), which may be operative in the design of FLN and whose exposition should have the potential to raise our explanation beyond explanatory adequacy.

2.1.2 The EPP

Let us now turn to the long-standing, recalcitrant case of the EPP: Does the EPP exist?, or more precisely, Are there any phenomena that cannot be accounted for without resorting to the EPP? The original formulation of the EPP limited its application to [Spec, T] (roughly, "a sentence must have a subject"). But it did not take long before an *generalized* (to some extent) version of the EPP emerged. Thus Chomsky (2000a) already observes that "Each core functional category (CFC) also allows an extra Spec beyond its s-selection: for C, a raised wh-phrase; for T, the surface subject; for v, the phrase raised by object shift (OS). For T, the property of allowing an extra Spec is the Extended Projection Principle (EPP). By analogy, we can call the corresponding properties of C and v *EPP-features,* determining positions not forced by the Projection Principle. I will restrict attention to XP positions, though a fuller picture might add X^0 as another case of the EPP. EPP-

features are uninterpretable (nonsemantic, hence the name), though the configuration they establish has effects for interpretation" (Bošković and Lasnik 2007: 212). Chomsky (2000a) further proposes the following principle, adding that "The fact that the EPP-feature when available is optional for C/v suggests that it is a property of the phase Ph" (C and v being phases — *NS*):

(7) The head H of phase Ph may be assigned an EPP-feature.

Chomsky (2000a) goes on to remark in his note that " ... I will call the EPP-feature a *P-feature (periphery feature)* if H does not have an appropriate EPP-feature by virtue of its inherent properties (e.g., the Case/agreement properties of v, the Q-feature of interrogative C). The device is introduced to extend the general theory of movement beyond A-movement, ···" (Bošković and Lasnik 2007: 214-215).

Discussing asymmetries between A- and A'-movement with regard to local steps, Chomsky (2005b) observes that reconstruction effects are far weaker for A-movement, that the only strong argument for local steps for A-movement based on binding and extraction facts is not so conclusive (with the effects holding only at [Spec, T] in both cases), and that there may be a plausible reason for believing that A-movement never passes through intermediate positions, though with some counterevidence still remaining. Chomsky (2005a: 10, 19, 22) talks of the *(still mysterious) condition EPP* and the *mysterious property of EPP*, admitting at the same time the existence of a *residual EPP effect* and exploring a possibility of reformulating the EPP in terms of inheritance of an edge-feature, the edge-feature (EF) of a lexical item (LI) permitting it to be merged (with, for example, EF (automatically available for an LI) of C attracting the *wh*-phrase to the edge of C, and with one of the properties of EF being that it permits free Merge to the edge, indefinitely).

Let us see the following interesting observation of Chomsky (2006: 17) on some characterization of the A/A'-movement distinction particularly from the perspective of the SMT: "A-movement is IM (internal Merge — *NS*) driven by uninterpretable inflectional features, while A'-movement is IM driven by EF of P (a phase head, C or v*; also see Chomsky 2006: 17-18 for a proposal that nominal phrases with D be phases — NS). Under the assumption that Agree (ϕ-/Case valuation) and the EPP are divorced, thus making long-distance Agree possible, and the additional assumption that the wh-checking mechanism (with the uninterpretable feature <Q> of a complementizer (the probe) locating in its domain the *wh*-phrase with an uninterpretable feature <wh> and an interpretable feature <Q> (the goal); see Bošković and Lasnik 2007: 83: from Chomsky 2000a) and the EPP are also divorced, it should be totally contingent on the EPP (a crosslinguistic parameter governing the (un)availability of every Spec and head position, perhaps with the (un)availability of complement positions regulated largely by θ-properties of lexical items; see Suzuki 2002 for the concept of *EPP-parametrization*) whether or not a specific Spec or head is employed (either overtly or covertly) for the purposes of sentential derivation at hand. Capitalizing

on the presence, among others, of the *defective intervention effects* (due to which effects of matching are blocked between α and γ in the configuration in which $\alpha > \beta > \gamma$, where $>$ is c-command, β and γ match the probe α, and β is inactive (the ϕ-/Case valuation of β having already been finished)) both in A-movement and in *wh*-movement (a species of A'-movement; with the *wh*-island condition as an instance of defective intervention effects), I would divide the whole complex above (comprising the A-/A'-movement parts) into two domains at a place different from the one in Chomsky (2006) above. A relevant division should be between what is involved up to the "force-finite" system, which may reside in some *lower* areas of the (generalized) C system (see Rizzi 1997), and everything else up there, which may include such pragmatic/discourse concepts of Topic, Focus, etc. Roughly, then, we obtain the two domains: one where IM is driven by uninterpretable features (up to the force-finite system in the sense of Rizzi 1997; crucially including *wh*-movement) and the other where IM is driven by EF of P (see above and Chomsky 2006: 17). Moreover, Chomsky (2005b: 17) observes with regard to IM in terms of EF of P that "Suppose that the edge-feature of the phase head is indiscriminate: it can seek any goal in its domain, with restrictions (...) determined by other factors. Take, say, Topicalization of DP. EF of a phase head PH can seek any DP in the phase and raise it to SPEC-PH. There are no intervention effects, ..." It then should follow that while in the narrow domain Agree (ϕ-/Case-valuation) and *wh*-checking apply independently of the EPP, the direct consequence of applying IM driven by EF of P in the outer domain may be the immediate realization by the IMed element (largely, overt) of the position targeted by EF of P, which I take to a most transparent, straightforward instantiation of the EPP-parametrization in the sense of Suzuki (2002). As for the very realization of a number of functional projections in the outer domain, you can specifically resort to at least two families of approaches to it: (i) the cartographic approach in the sense of Rizzi (1997, 2004), etc., where "The relation between syntax and the interpretation interface (LF) is expressed in an optimally simple way: the interpretation is read off the syntactic configuration" (Belletti 2004: 17); and (ii) an approach of the sort that makes an important distinction between what is obtained grammar-internally and what is not , such as discourse entities/notions in particular, like that of Gill and Tsoulas (2004), where an extremely interesting attempt is made to capture the notion and interpretation of Topic in Korean without resorting to elaborate peripheral syntax (see Suzuki 2005: 79-81 for some related discussion and the problem (with its tentative solution) arising from the seeming impression that the cartographic approach better fares in connection with the SMT).

Notice that the traditional statement itself of the EPP to the effect that "a sentence must have a subject," roughly in the sense that [Spec, T] must somehow be filled/realized (either overtly or covertly), should simply be not correct, given crosslinguistic, empirical evidence and its plausible analysis of Alexiadou and Anagnostopoulou (1998). Alexiadou and Anagnostopoulou (1998) is an interesting attempt to argue that the way the EPP is checked (via move/merge XP vs. move/merge X^0, the latter satisfying the EPP via V-

raising) and the parametric availability of [Spec, T] for subjects may play major roles in accounting for a number of intriguing, parametric differences observed across Germanic, Celtic/Arabic, Romance, and Greek (see Alexiadou and Anagnostopoulou 1998 for details). It should already be fairly easy to see that there *are* languages without [Spec, T] from the very assumption (if correct) by them (1998) of the parameter regulating the presence vs. absence of such a structural position. Based on the two major parameters above, the EPP/AGR parameter and the [Spec, T] parameter, Alexiadou and Anagnostopoulou (1998) define four language-types with distinct properties. The two types which concern us here are one of the move/merge XP plus the unavailability of [Spec, T] and the other of the move/merge X^0 plus the unavailability of [Spec, T], the former including languages such as English and the latter languages such as Greek and Romance (see Alexiadou and Anagnostopoulou 1998 for further details and examples).

Suzuki (2002: 19-24) proposes as an alternative to the traditional EPP in its original sense what is called *EPP-parametrization*, which is intended to be a "maximally generalized version of the EPP" that may encompass possibly a large class of parameters associated with the EPP-feature in that it regulates the possibility of the realization (either via IM or via EM (external Merge)) of various functional positions (both X^0 and XP) a la Cinque (1999) with (phonological) material or, perhaps, with UG-provided empty categories. To put it more simply, EPP-parametrization with the single "selectional" EPP-feature regulates the possibility of the realization of all syntactic positions, perhaps apart from the realization of complement positions, which should be a matter of concern on the part of θ-properties of lexical items. In the terminology of Chomsky (2006), where one of the important assumptions is that the EPP applies independently of other operations such as Agree, wh-checking, etc. (see above), we can easily see that EPP-parametrization applies both in the inner domain where IM is driven by uninterpretable features (up to the force-finite system; crucially including the case of wh-movement) and in the outer domain where IM is driven by EF of P.

Finally, let us turn to the extremely intriguing problem of the (un)availability of the EPP in some ellipsis contexts. Based on Merchant (2001), Craenenbroeck and Dikken (2006) argues that the idea that ellipsis suspends the requirement that the subject raise to [Spec, T] (as seen in the phenomenon of the lifting of the subject condition effects under sluicing) can be accounted for in terms of the EPP *as a PF condition.*

(8) a. *Which Marx brother is [a biography of (which Marx brother)] going to appear/be published this year?
b. *Which Marx brother did [a biography of (which Marx brother)] cause a scandal earlier this year?

(9) a. A biography of one of the Marx brothers is going to appear/be published this year — guess which (Marx brother).

b. A biography of one of the Marx brothers caused a scandal earlier this year, but Bill doesn't recall which (Marx brother).

Roughly, Merchant's (2001) answer (with the assumption that the EPP is a PF condition) to the problem runs as follows: under the assumption that the subject condition is a constraint barring extraction out of the subject of a finite clause in its derived position, there should be no ban on extraction from the subject when it is in its vP-internal base position and in (9a,b) *which (Marx brother)* indeed extracts from its containing NP while the latter is inside vP, thus the EPP (requiring the subject to raise to [Spec, T]) being suspended (see Craenenboeck and Dikken 2006: 654-655 for further details concerning the treatment of the EPP as a PF condition).

I would like here to account in terms of the top-down approach to narrow syntactic derivation (see Phillips 1996, 2003, Terada 1999, 2002; Reinhart 2006 as well) for differences in grammaticality and other matters between the ungrammatical (due to the subject condition) overt wh-movement cases in (8) and the grammatical sluicing cases in (9). Consider first the overt wh-movement cases in (8a,b), which are violations of the subject condition barring extraction out of [Spec, T] of a finite clause:

(10) Which Marx brother ... [$_{TP}$ [a biography of (which Marx brother)] (... ([$_{vP}$ [a biography of (which Marx brother)]) ...)

The structure in (10) applies to both (8a,b) (only the relevant portions being represented in a rather simplistic fashion), and the parentheses enclosing vP indicates that vP would not be pronounced. Starting with the sentence-initial *which Marx brother*, the top-down parser/producer, which seeks its launching site in its c-command domain (with the same set of grammatical devices as in the case of the standard bottom-up approach in place and available and, perhaps, with an additional device such as "reanalysis" playing an important role, as in Reinhart 2006), looks "downward" and reaches the prospective NP in [Spec, T] crucially earlier than the NP in [Spec, v], which is possible due to the presence of c-command relationships between elements in the structure. We appear to encounter a configuration akin to one relevant to the *defective intervention constraint (DIC)* to the effect that in the structure "$\alpha > \beta > \gamma$", where > is c-command, β and γ match the probe α, but β is inactive, the effects of matching between α and γ are blocked. I claim that defective intervention effects of some sort may be observed in (10), where *which Marx brother* > *a biography of (which Marx brother)* in [Spec, T] > *a biography of (which Marx brother)* in [Spec, v], a biography of *(which Marx brother)* in [Spec, T] is "inactive" (due to the subject condition/because of its status as an island), and hence *which Marx brother* cannot look down into [Spec, T] or reach *a biography of (which Marx brother)* in [Spec, v] for the purposes of locating the launching site. From the perspective of the top-down approach to syntactic derivation, the *wh*-phrase in [Spec, C] cannot look down below the

NP in [Spec, T] for the purposes of seeking its original position, leading to the cancellation of the derivation at this point. Let us then turn to the portions that contain a sluiced part in (9a,b) where we observe a seeming violation of the subject island condition:

(11) ... V [which (Marx brother) { C {(a biography of which Marx brother) { T ... {(a biography of which Marx brothers){ v+V ...}}}}

Note that V, C, T, and v in the representation (albeit interpreted phase-derivationally, as has been presupposed throughout) in (11) are abbreviations/mnemonics for relevant lexical items/features and that the braces ("{ }") indicate that elements enclosed in each pair of them are *sets* (crucially, lacking any linear order and hence, any asymmetric c-command in the sense of Kayne 1994, between the two elements in every pair of braces, keeping to binary-branching phrase structure for human language in the sense of Kayne 1984). I assume that the whole internally unordered set without internal hierarchy (but with labels in place) consisting of such binary sets may amount to the narrow syntactic component of FLN (see Chomsky 2004 and much recent work). As for *labels*, I continue to assume their existence or that of their residues, roughly following Uriagereka and Pietroski (2002: note 12) (somehow contra Collins 2002; see also Chomsky 1995b and much subsequent work for further details and developments; note also the interesting assumption of Uriagereka and Pietroski 2002 that adjuncts do not have labels, another indication of dimensional differences between adjuncts and arguments (see part 5 below of this paper)). I further assume that the braced portions in (11) are *pure narrow syntactic elements*, albeit crucially with labels, but without asymmetric c-command or linear order, which latter two concepts I assume, roughly following Chomksy's (1995a: 413-420) original argument, to be resources belonging in the phonological component, (part of) the mapping mechanism to the interface PHON. Abstracting away from the exact mechanism of intersentential rule of sluicing, our top-down parser/producer first encounters *which (Marx brother)* (of the relevant part of (9a,b)/(11)), then seeking "downward" for its launching site for the purposes of its theta-theoretic interpretation. It may encounter a number of two-membered sets with their members consisting of a single lexical element or of more than one such element, but, crucially, there are no hierarchical relations such as (asymmetric) c-command or linear order between members of such sets and hence, between sets themselves, with the sole exception of the presence of labels, as noted above. (The parentheses enclosing the two instances of *a biography of which Marx brother* in the partial representation in (11) are intended to show that they are not pronounced, a rather redundant consideration, given the very constructions with a sluiced portion in them, but I leave them as they are for expository purposes.) Assuming that our top-down parser/producer cannot look into the part in question (*a biography of which Marx brother*, co-member with a T-projection in some set) because its status as co-member with a certain T-projection leads to the conclusion that it is a (subject) island, leaving it frozen for the purposes of further computation,

it still can seek elsewhere and look into the other instance of *a biography of which Marx brother* and reaching its original/pre-*wh*-movement position for the purposes of its theta-theoretic interpretation, crucially because it is not an island (by hypothesis) since it is co-member with a v-projection, but not with a T-projection, in a set, and there are no c-command relations between sets in the representation in (11), which is assumed above to be a narrow syntactic one, and hence, no possible defective intervention effects of a sort involving the two instances of *a biography of which Marx brother* under discussion.

As for the concepts of (asymmetric) c-command and linear order, I assume much in the spirit of Chomsky (1995a) and much subsequent work that they should properly belong in the phonological component, (part of) the mapping-interface portion of FLN. Actually, the transition from the narrow syntactic "set" structures to possible configurations instructed by such concepts would be extremely straightforward, but we somehow wait until the phonological component (presumably, in connection with the which-copy-to-pronounce rule, the phonological cliticization rule, etc.; see Suzuki 2005, 2006; and until the semantic component, for binding and QR purposes, for example), simply because they are not needed in narrow syntactic derivation (due to economy considerations). I have adopted in the exposition here of (8) and (9) the stronger assumption concerning the putative rule of sluicing that the elided portion in the sluiced construction continues to retain both the structural and semantic resources, which may enter into a relation of some sort involving some concept of parallelism both in structure and semantic interpretation with its antecedent, with only the phonological part (i.e., "sound" resources including the phonological component of FLN, namely, the mapping mechanism to the PHON interface, and much more) missing from the construction. Finally, recall again that we have accounted for the problem of ellipsis (sluicing) and EPP repair raised by Craenenboeck and Dikken (2006) from the perspective of the *top-down approach to syntactic derivation*, even without being involved in the very notion of the EPP, and hence without any complication or concern over such a dubious move as the lifting of the subject island condition (resulting from the assumption that the EPP is a PF condition), given also the validity of such a movement framework as that of Richards (2001), where overt and covert movement are treated alike with regard to syntactic principles constraining movement.

3 Helen Keller

3.1 A Short Introduction to the Problems Raised by Gill (1997)

The third chapter of Gill (1997) is devoted to the discussion of the case of Helen Keller from the perspective of some species of body theory, arguing at the same time both against Skinnerian behaviorism and Chomskian nativism. Gill's (1997) basic stance with respect to human cognition in general can best be detected in the following observation: "... Perhaps

the best place to begin is with the thoroughly embodied character of human experience and speech. Traditional philosophy has systematically ignored the role of the physical dimension in human cognitive activity, including its linguistic aspects. Knowing and thinking are generally held to be exclusively matters of the mind, with language being simply the external symbolization thereof. If any single truth has emerged from the investigations discussed in this book, it is that this intellectualist perspective is entirely wrong-headed, or better, entirely out of touch with the facts concerning human cognition and speech. It is clear that those who come to know their way around in their mother tongue, both in understanding and expression, do so by means of bodily action and interaction. In addition to the physical aspects of hearing and speaking being crucial to such knowledge, the actual grasping of the dynamics of thought and language is mediated in and through the axis provided by the embodied character of human existence (pp.144-145)." Notice that these remarks seem to be made on the tacit assumption of the validity of the long-practiced, traditional concept of dualism with the dichotomy between the mind and the body throughout most of the history of modern philosophy. Chances are that proponents of what are called "body theories" specifically came to exist or the number of such people suddenly came to increase roughly at the time when Descartes's famous declaration of the significance of the mind/mentalism began to take shape in philosophical theories. Presumably, body theorists have fought the general tendency to make more of the mind than of the body for no obvious reasons (recall the famous phrase: "I think; therefore I am"; also Pascal's "Man is a thinking reed") which has long been predominant in thought, philosophy, and elsewhere (note the current western medical practice to take the brain death to be human death irrespective of the body condition; to which I return below). But recent brain and neurological studies have claimed for some time that what we call thoughts, feelings, and the like (i.e., the mental) simply arises from a variety of chemical interactions of neural tissues in the brain among themselves, leading to the idea that (the contents of) the mind may be physically based and that everything human (i.e., the body and the mind) must be understood in terms of the concept of physicality. Can it then be possible to treat everything human exactly on a par? I propose that the relevant dichotomy be between the subconscious (roughly, the "Unconscious" in the sense of Freud) and the conscious (with the overall picture here realized along the lines of Freudian psychoanalysis), this new kind of dualism crosscutting both traditional domains.

Before looking at problematic (to us) places in the third chapter of Gill (1997) one by one and reconsidering each of them in terms of Chomskian nativism and providing them with an alternative on our part whenever possible, it should be pertinent to take up the following remarks in Gill's (1997) own introduction of his third chapter: " ... Once again, the similarities and differences between and among Keller, the various 'wolf children,' and the chimpanzees are fascinating and significant. Her linguistic success, in contrast to the near total failure of the feral children, even though they possessed essentially the same genetic endowment, stands out as strongly as her ability to outdistance the minimally

successful chimpanzees. She achieved in a month what the chimpanzees needed years and even 'normal' children take many months to accomplish. Moreover, Keller went on to linguistic levels far beyond those of most human adults (p. 6)." In order to precisely assess in our terms the linguistic abilities of the three parties concerned (i.e., Keller, wolf children, and chimpanzees), we have at least to closely examine the contribution (if any) made to each case on the part of FLN, in addition to the important consideration of the linguistic stimuli/triggers after birth. (Notice that Chomskian *nativism* requires *two* kinds of ingredients, i.e., the genetic endowment *and* the after-birth stimulus/trigger, for the successful explanation of the fascinating feat of language acquisition, while both Gill's (1997) body theory and Skinnerian behaviorism seem to require only one condition of 'linguistic experience, training, etc. after birth,' presumably strengthened by parental reinforcement in the latter case, for the purposes of explicating language learning. See also Lightfoot 1994 for an interesting characterization of triggers as an indispensable ingredient for first language acquisition.) The first major distinction should be made between Keller and wolf children, on the one hand, and chimpanzees, on the other, the former arguably possessing FLN and the latter lacking it (although both are assumed to possess the faculty of language in the broad sense (FLB); see Hauser, Chomsky, and Fitch 2002 for details). According to Hauser, Chomsky, and Fitch (2002: 1569), the concept of recursion (and the mechanism of "mapping to the interfaces") may constitute the whole empirical content of FLN and the latter faculty must be the only uniquely human component of the faculty of language. It may then be clear that "her (Keller's — *NS*) ability to outdistance the minimally successful chimpanzees (Gill 1997: 6)" should come as no surprise, given the assumption above that chimpanzees do not possess FLN, whose property of recursion must be responsible for the possibility of producing (infinitely) multiply embedded sentential structures, for example. Far minuter subtleties, however, may have to be detected in considering the differences in linguistic ability between Keller and the feral children (including Genie) since both possess FLN (and FLB). This situation should mean that both parties concerned are innately (i.e., in regard to one condition requisite for successful first language acquisition) on a par with each other, forcing us to seek possible differences between them with respect to the other crucial ingredient for acquisition (i.e., after-birth stimulus/trigger). Recall that Helen Keller had been growing up as a "normally developing child" until she fell sick at the age of one year and nine months with a high fever and stomachache for which there was no obvious cause, ending up as one who was deaf, blind, and mute. The most important point in the discussion here is that "Keller had been growing up as a normal child up until the age of one year and nine months," and many biographies of Keller assert that she had been a very bright girl, being far ahead of her friends of her age in various abilities. Note that Wexler, Schutze, and Rice (1998: 321) observe based on Wexler (1994) that "children know the correct grammatical features of basic clause structure and inflectional morphemes, including properties that dictate whether a particular language shows verb movement, and so on," claiming further on the

basis of the hypothesis (*"Very Early Parameter Setting"*) proposed by Wexler (1996, 1998) that "Children normally set their basic clause structure/inflectional parameters correctly at least from the time they begin to produce two-word utterances" and adding that " ... children are outstanding learners of clausal and inflectional properties: they do it well and they do it before they have a chance to be corrected for making mistakes (i.e., before they produce the constructions)." It can then safely be said on the basis of the observation above concerning Keller's childhood in which she had been a normal, bright child (till she fell sick) and of results of various studies in parameter setting (specifically, Wexler's (1996, 1998) principle of "very early parameter setting") that the head-parameter (i.e., with two options VO vs. OV) and the Verb-raising parameter (with two options ±V-raising), for example, had already been set since (I suspect) she must already have entered the two-word utterance stage (before she fell sick), when the major properties of the VO option and the −V-raising option (for English) must have abundantly appeared in various phrasal structures that she produced (see Guasti 2002: 101-150). Notice that nothing like these ever happened to the feral children obviously in the absence of any after-birth linguistic stimuli/triggers (i.e., one of the two ingredients for successful first language acquisition), under the assumption that they had not received any meaningful linguistic stimuli before they turned wolf children and also that they did not have any opportunity to have mates to "talk with" after they became feral. The latter condition of having mates to talk to was apparently satisfied in the case of deaf children mentioned in Crain and Lillo-Martin 1999 who had developed "home signs" to communicate among themselves during the major part of the important critical period, to which I return below. Note that the discussion here ignores largely for lack of data or evidence possibilities of these children having been exposed to any language triggers before they became feral. Recall also the case of Amala and Kamala (see Gill 1997: 36-38) for some variation in age at which they were taught language *for the first time* after they had been found and brought back to the human community, the former girl Amala, the younger of the two, being about one and a half years old at the time of their discovery in 1920 by the Reverend Singh and dying after one year from nephritis and generalized edema, and the latter Kamala, the bigger of the two, being at least eight and also dying of the same condition roughly eight years later. Presumably, one of the most straightforward modes of explanation of the fact that they had not developed "language" in whatever form during their life as "wolf cubs" may be that they were too far apart in age to be "talking mates," inviting us to conjecture that if they had been sufficiently near to each other in age, they (being the only humans with FLN among the wolf group) might have developed an (even spoken) species of "language" roughly in the fashion of the deaf children who developed "home signs" *spontaneously* among themselves, and that both Amala and Kamala arguably lacked after-birth linguistic triggers (i.e., one of the two crucial ingredients for first language acquisition), the bigger girl having presumably been alone with nobody to talk to her during the critical period and the smaller one (even if currently passing through the sensitive period at the

time around their discovery) also having nobody to talk to her (with Kamala unavailable as a trigger provider or a talking mate). Note that all the discussion so far is based on the assumption of the validity and viability of Suzuki's (2006) "lexical condition on first language acquisition after birth," which limits the object of the child's after-birth acquisition to the lexicon/lexical items of her target language. And another more interesting problem/puzzle with language may be that human language cannot possibly be acquired alone (i.e., with only the learner present on the acquisition scene); that is, the learner must be helped in (specifically, linguistically) particular ways by other humans (or by at least one other human). Roughly, some keys to its solution may lie in the following assumptions (to which I return later on): (i) the most crucial requirement in this situation should be that both the learner and the helper(s) must have FLN, that is, both parties concerned here must be *Homo sapiens* (with the possession of FLN as one of its defining properties); (ii) if one of the indispensable requirements imposed on the first language learner is to reach FLN, it can only be accomplished through bootstrapping (in the original sense of Pinker 1984; see also Dresher and Kaye 1990, Dresher 1999, Suzuki 2002 for further details), which mechanism is arguably biologically (hence, uniquely humanly) innate; and (iii) since arguably "abstract" FLN can only be reached through bootstrapping with the aid of the arguably "accessible" (to the learner, though maybe in some receptive manner, at first) interface properties, the learner definitely needs somebody other than herself to provide her with such interface properties that are the sole means of arriving at FLN, *simply because* she herself does not have any abilities yet to present/produce such interface properties in the form of "sound/sign," "meaning," or whatever.

3.2 A Critical Examination of Gill's (1997) Arguments against Chomskian Nativism

Based on the discussion in section 3.1 above where I have shown in the framework of Suzuki (2001, 2002, 2003, 2005, 2004) the general directions in which to proceed to solve the various problems raised in terms of Gill's (1997) "body theory," let us get on to the critical examination of the arguments against Chomskian nativism seen in the third chapter of his book one by one.

(12) < *Gill's (1997) problematic argument 1* >
"Chomsky has argued that we are born with a universal grammar, one that operates according to certain transformational rules already implanted in the circuitry of our brains" (p.50)

As has always been and presumably still is the case in most instances of critical discourse against Chomskian generative grammar, Gill(1997) also seems to base his criticism on some parts of Chomsky (1965). (Although Gill 1997 only includes Chomsky 1975 in his

bibliography, I believe that the general ideas and specific implementation of grammatical devices alluded to in his third chapter should largely be ascribed to Chomsky 1965.) A more precise wording of Gill's argument above may be that "Universal Grammar (UG) that we are born with (UG being the linguistic characterization of FLN at the time of birth) has a set of (innate) transformational rules as one of its components (phrase structure rules and the lexicon belonging to UG as its other components)" (see Chomsky 1981, 1995 for much more on the concept of transformation).

(13) < *Gill's (1997) problematic argument 2* >
"Thus, all natural languages are the product of a common 'deep structure' that represents the 'language of thought' itself" (pp.50-51).

The long-standing claim on Chomsky's part that it may not be the case that the primary raison d'être of language is to be used for communication purposes points to the validity of the wording "language of thought" in Gill's argument above (pp. 50-51). The claim is repeated in the current minimalist assumption that FLN, the uniquely human endowment, had evolved, specifically not for the purposes of communication (see Fitch, Hauser, and Chomsky 2005). The most serious problem with Gill's Argument 2 seems to reside in its following portion: "... all natural languages are the product of a common 'deep structure' ....." To the best of my knowledge, Chomsky himself has never said or written or endorsed anything of the sort (at least publicly) throughout his career as a generative linguist. (As Chomsky 1972b: 115 admits, Chomsky 1965 was among the several frameworks of transformational generative grammar where only deep structure was responsible for semantic interpretation. Chomsky 1972b: 117 already observes largely on various empirical grounds that a grammar where surface structure as well as deep structure plays a role in semantic interpretation may be preferable to one where only deep structure is responsible for it.) But, of course, something of the sort *was* entertained by some people, and we know that taking natural languages this way had a certain amount of influence particularly in the late 1960s and early 1970s, when perhaps by far the strongest animosity existed between Chomsky (and his supporters) and his opponents, specifically generative semanticists. The latter people were staunch advocates of the standard theory of generative grammar as formulated in Chomsky (1965), where deep structures were (totally) responsible for meanings (of sentences), while surface structures determined their pronunciation (see Chomsky 1972, 1977; Jackendoff 1972, among others, for a number of problematic aspects of the standard theory model, further details of the controversy, and subsequent developments of generative grammar). The problematic portion of Gill's (1997) Argument 2 above appears to reflect the alleged practice of the standard theory of Chomsky's generative grammar, but see the following, more precise wording: "... a sentence with (roughly) the same meaning of all natural languages is the product of a deep structure common across all natural languages ...."

(14) < *Gill's (1997) problematic argument 3* >

"The difficulty with all this, however, is that in addition to the fact that these universal rules remain elusive to the best efforts of Chomsky and his cohorts, the entire schema is presented as if human speakers were not embodied. Human physicality and activity are simply given no place in the entire process of language acquisition" (p.51).

It may indeed have been true that "these universal (transformational) rules" have remained "elusive" for a long time. But I suspect that one of the strongest reasons for their "elusiveness" has been that people, specifically those in the empiricist tradition, have been obstinate in their requisition on Chomskian nativism (which is in the rationalist tradition) for observable (in quite a narrow sense, in my judgment) evidence for these rules (see Halle, Bresnan, and Miller 1978 for some details of the "psychological reality" controversy, for example) and quite reluctant to admit even the widely accepted linguistic facts based on the well-established literature (see, for example, Grodzinsky 2006 as one of the most recent discoveries in the generative grammar tradition in conjunction with recent results from brain sciences for the hypothesis that Broca's area on the left hemisphere in the human brain is responsible for syntactic movement, i.e., transformation). So the alleged elusiveness has never been a real problem for us generative linguists and acquisitionists.

(15) < *Gill's (1997) problematic argument 4* >

"While Skinner allows speech to be overdetermined by somatic factors, albeit in relatively passive fashion, Chomsky requires it to be overdetermined by intellectual principles" (p.51).

While it is not very clear what exactly Gill means by the expression "overdetermination," I would like to interpret "overdetermination" here as "sufficient determination" for the purposes of discussion (because the term "overdetermination" usually implies the situation where the grammar generates an extra set of sentences which is not necessarily grammatical or rather ungrammatical since it is arguably outside the set legitimately generated by the grammar, in addition to the set legitimately allowed by it, leading to the suspicion that such a grammar may not be a right one as it generates ungrammatical sentences as well). I would also interpret Gill's (1997) "intellectual principles" as "innate grammatical principles that are part of UG." Now, the question here is: "Does Chomsky require speech/language to be sufficiently determined by innate UG principles (alone)?" Definitely not. Recall that as noted in section 3.1 above, Chomskian nativism requires two kinds of ingredients for the explanation of successful first language acquisition: i.e., the genetic endowment and after-birth triggers. Gill's (1997: 51) "intellectual principles" (interpreted as "innate UG principles") should arguably constitute part of the human genetic endowment for the purposes of language.

Gill (1997: 51) goes on to point out that "Her (Keller's — *NS*) achievement is particularly significant because she came to experience meaning and to know the world around her by means of *tactile sign language* (emphasis — *NS;* to which I return below)" and to correctly observe that "Although her (Keller's — *NS*) situation as a deaf-mute was uniquely dramatic and atypical, it was essentially no different from that of all children who become members of a speaking community through verbal interaction with those around them."

Gill (1997: 51) observes in connection with Skinnerian behaviorism that " ... it (Skinnerian behaviorism — *NS*) has no way to account for her (Keller's — *NS*) ability to move beyond the apelike stage of imitation with respect to the sign language to which her teacher, Annie Sullivan, had introduced her." Recall that the "sign language to which her teacher, Annie Sullivan, had introduced her was a "tactile sign language." And while I am not sure on what level of sophistication Gill considered the "apelike stage of imitation" to be, Hauser, Chomsky, and Fitch (2002: 1575-1576) make somewhat detailed remarks about abilities in imitation, including animals other than humans and apes and covering imitation in more than one modality. According to Hauser, Chomsky, and Fitch (2002), the human capacity for (vocal) imitation, which was a crucial prerequisite of FLB as a communicative system, is obviously indispensable for the acquisition of the lexicon. They go on to point out that vocal imitation is not uniquely human, rich multimodal imitative capacities being seen in dolphins, parrots, and most songbirds, and according to them, what is striking is the virtual absence of evidence for vocal imitation in either monkeys or apes. As for usually mediated imitation, both monkeys and apes do not show much evidence of capacities for it. Compared with parrots that can readily acquire a large vocal repertoire, even intensively trained chimpanzees are incapable of acquiring anything but a few poorly articulated spoken words, leading to the suggestion that they lack a vocal imitative capacity. And while chimpanzees may be able to learn several hundred hand signs with persistent training, evidence for spontaneous visuomanual imitation in them is generally sparse. Hauser, Chomsky, and Fitch (2002: 1576) further observe in connection with modality-specificity that " ... even in cases where nonhuman animals are capable of imitating in one modality (e.g., song copying in songbirds), only dolphins and humans appear capable of imitation in multiple modalities. The detachment from modality-specific inputs may represent a substantial change in neural organization, one that affects not only imitation but also communication; *only humans can lose one modality (e.g., hearing) and make up for this deficit by communicating with complete competence in a different modality (i.e., signing)"* (emphasis — *NS*).

(16) < *Gill's (1997) problematic argument 5* >

"Keller learned many signs and could 'repeat' them in relation to their appropriate objects fairly consistently. At first, however, she did not seem to 'understand' anything that Sullivan signed to her, nor did she ever initiate any signing in order

to 'say' anything herself. ... Nevertheless, one day by the pumphouse something more than mere imitation and conditioning transpired" (p.51).

Let us first make sure that it was in 1887, when Keller was seven years old, that Annie Sullivan, an extraordinary teacher, came to her house as governess and that "many signs" that Keller learned must have taken form largely in *tactile* memories as a result of the teacher finger-spelling into Keller's hand while putting her in physical contact with some object in the world around her. (Note that the latter way on the part of Sullivan of helping Keller learn words should be one giving "ostensive" definitions, which Gill 1997: 54-55 comments on by observing that " ... the activity of giving ostensive definitions is itself so fundamental that it cannot be taught by ostensive definition; one cannot teach it by pointing at it!" And, of course, receiving ostensive definitions of words is definitely not a/the normal way in which the child learns the meanings of words of her first language. Consider the seeming impossibility of teaching children how to use and understand relative words such as "here," "there," "this," "that," "yesterday," and "tomorrow.") And I here take the observation in Argument 5 that "Keller ... could 'repeat' them in relation to their appropriate objects fairly consistently" to point to the situation (before the "pumphouse incident") where she (correctly) connected objects with appropriate words/-names largely via the general learning strategy (GLS). According to Helen Keller Campaigning Committee/*The Mainichi* (1948/2003), however, Keller came to understand that everything has a name in the second week since her teacher's arrival and she learned 300 words within three months of Sullivan starting her education. While Keller's recognition of everything having a name may better be treated in the broader context of cognition, the fact that she learned 300 words in three months should be dealt with in a more or less linguistically acquisitional context. This achievement of Keller's in word learning may amount to learning 3 or 4 words a day, which is far inferior in the number of words learned to the normally developing child learning around 10 words a day during the critical/sensitive period. Notice that Keller was already seven years old when Sullivan came to her house in Tuscumbia, Alabama, which may have been a little bit too late for her to acquire her first language biologically, in view of the standard assumption that the critical/sensitive period for language closes at about the age of 5 or 6 years (with some reservations concerning much recent care not to take the period to be an absolute one; hence, the newly adopted name of the "sensitive period"). However, I would like to claim that there must have been a major difference in Keller's language ability between the time intervals before and after the pumphouse incident, in which "something more than mere imitation and conditioning transpired." I would conjecture that although Keller fell sick with a high fever and stomachache for which there was no obvious cause, ending up as one who was deaf, blind, and mute, the very fact noted above in sections 1 and 3.1 that she had been growing up as a normally developing child until the age of one year and nine months should play a major role in exploring the true reason(s) for her subsequent fantastic

achievement. Recall Wexler's (1996, 1998) hypothesis of "Very Early Parameter Setting" to the effect that "Children normally set their basic clause structure/inflectional parameters correctly at least from the time they begin to produce two-word utterances" (see section 3.1). This should invite us to assume that most major syntactic parameters pertaining to clausal structure for English, Keller's first language (a spoken language), must already have been set (arguably) correctly before she fell sick at the age of one year and nine months (see the discussion on the head-parameter and Verb-raising parameter in connection with Keller's acquisitional situation in section 3.1 above). Keller must also have learned a good number of words before she fell sick since "at about 20-24 months children experience a vocabulary spurt, learning between five and nine new words a day up to the age of 6 years" (see Guasti 2002: 81). I conclude here that before she fell sick at the age of one year and nine months, Keller had reached the stage of language development when most major ingredients needed for the purposes of successful first language acquisition were in place, but that her serious disease, which left her deaf, blind, and mute, also had the deadly consequences of driving her language capacities into a frozen state, that is, into a state in which there were in large measure no ways of access to them from outside. Some of my assumptions in this discussion are that Keller's language abilities (*competence*, to which I return, in the sense of Chomsky 1965; see also Suzuki 2006) may not have been damaged, being left in its near complete form (otherwise we would not be able to explain her subsequent feat, under the factual assumption that the language that Keller ultimately acquired was English, but not a (largely manually) signed language such as American Sign Language (ASL; notice that Keller was blind) and that Keller learned English as a *spoken* language, at least for the purposes of production, as many parents of deaf (but not blind, in the usual case) children used to or still do hope that they would/will successfully learn a spoken language), and that it may have been only her *hardware* (relevant to performance; see Chomsky 1965, Suzuki 2006) that may have been damaged. But this damaged linguistic hardware seems to have had the effect of partitioning part of Keller's life pertaining to language into the following two time intervals: (i) the interval from the time she fell sick at the age of one year and nine months through the pumphouse incident, when her performance continued to be separated from biolinguistic resources in her brain and was in large measure GLS-guided; and (ii) that from the pumphouse incident onward, when biological resources in her brain that had been acquired while she was a normally developing child before her disease were somehow accessible to outside uses (albeit in quite a different fashion due to the hardware damage), specifically with FLN in place and biolinguistically operative.

(17) < *Gill's (1997) problematic argument 6* >

" ... Chomsky's structuralism fails to explain Helen Keller's achievement because it separates it from her embodied behavior. To say that acquiring one's natural language is more than mere behavior does not entail that it is less. In fact, to

> extricate Keller's breakthrough from its physical and social setting by focusing on an intellectual pattern, as Chomsky's theory necessitates, is to render it inexplicable in principle as well as humanly unrealistic. The acquisition of language is neither exclusively a function of the body nor one of the mind; rather, it is the result of the interaction between the two in a concrete and evolving context" (pp.51-52).

Here again Gill (1997) reiterates that Chomskian nativism separates Keller's achievement from her embodied behavior, making sure at the same time that first language acquisition is more than mere behavior. As noted in section 3.1 above, the traditional kind of dualism with the dichotomy between the mind and the body (specifically, since Descartes and Pascal, for example) seems to have been questioned for some time and to have gradually been replaced by the more reasonable *dualism* with the dichotomy between the subconscious and the conscious as a leading tool of philosophical and scientific analysis and explanation. So faced with the dominant tendency of current scientific practice to take everything human (i.e., the mind and the body) in terms of the notion of physicality, I just note here that the first half of Gill's (1997) reiteration is simply extremely hard or even impossible to make any reasonable comment on due to the presence of a huge gap in background assumption between his position and Chomskian nativism (combined with the new species of dualism above). And the second half of his reiteration to the effect that language acquisition is more than mere behavior should be considered to be sheer uncontroversial fact, the more so because of Gill's concept of behavior in terms of the notion of the body. Recall here Fodor's (1998) notion of "learning by parsing" (see Suzuki 2002 for some discussion involving Fodor's 1998 structural triggers learner model and the access problem with Fodor's system), which may be a species of *performance* on the part of the learner; that is, the child has to learn her first language through performance of some sort in any case simply because she learns it while at the same time living in this world as a human individual (to which I return below, along with some discussion on Suzuki's (2002) concept of "bootstrapping as trigger"). I am afraid that the next portion in Argument 6 dealing with "extrication of Keller's breakthrough from its physical and social setting" may threaten to reveal lack of a clear understanding on his part of what scientific theorizing should be like. First, the scientific common sense has it that if you want to know the macrostructure, you will have to begin with the microstructure, since it is impossible to start with the macrostructure itself and arrive at any understanding of it.[1] Furthermore, Jenkins (2000: 102-104) cites Philip Anderson's (1972. More is different. *Science* 177(4047): 393-396) hierarchy, according to which a science higher up in the hierarchy obeys the laws of a science lower down (e.g., solid state physics obeys the laws of elementary particle physics), with entirely new properties (i.e., "emergent properties") appearing at each level of complexity. Gill (1997: 52) ends this (largely) problematic argument with the observation: " ... rather, it (the acquisition of language — *NS*) is the result of the interaction

between the two in a concrete and evolving context." It may indeed be true that both the mind and the body (in the traditional sense) interact in the feat of language acquisition, given the assumption above that there is no longer any viable notion of dualism with the dichotomy between the mind and the body. The phrasing "in a concrete and evolving context" appears to correctly point to the actual acquisitional situation where in our terms and in the case of spoken language the child usually hear language and language-like noise daily, with no other resources available to her than innate universal grammar to guide her through the fantastic feat (plus after-birth language input as stimulus/trigger).

(18) < *Gill's (1997) problematic argument 7* >

"Prior to this event (the "pumphouse incident" — *NS*), she (Keller — *NS*) was, in her own words, a mere 'phantom' who responded to tactile stimuli without ever engaging in an 'intentional' act as a human being. Though atypical in its specific form, her acquisition of language dramatizes the role of embodiment as well as that of speech in establishing the meaning of being human. For Helen Keller's language was distinctively, though not uniquely, a somatic affair, and thus, so was her cognitivity" (p.52).

My first general comment on Gill's (1997) Argument 7 above is again that his entire argument that is based on the notion of the body, which constitutes an indispensable part of the traditional dualism based on the dichotomy between the mind and the body, only puts me completely at a loss what to do, apparently thanks to differences in basic background philosophy (see Chomsky 2000b: 75-105 for arguments against dualism and in favor of what he calls "naturalism," which I believe should primarily be based on the notion of "physicality," noted in section 3.1). But I take up and discuss a little bit the expressions "phantom" and "intentional" appearing in the argument above. I would suspect that the word "phantom" was employed and used by Keller herself just as an afterthought (presumably, for lack of a better phrase) to describe her devastating condition, in which most hardware resources were severely damaged and most of the daily ways of self-expression were no longer available to her. Recall my assumption above in connection with Gill's Argument 5 that the pumphouse incident partitioned the relevant portion of Keller's life into two time intervals; i.e., one when her linguistic performance must have been guided by GLS-related means and the other when she had access to biolinguistic resources (again). Keller might have experienced in the former time interval irritation and impatience of the sort that you would have when you cannot possibly do what you want to, under the assumption that most of her software resources were largely intact. But chances are that she even could not experience such feelings, given the devastating damage (arguably) to the hardware, which must be (at least partly) responsible for the perception of such emotions; hence, the phrasing "phantom." "Intentinal" acts were also impossible for Keller to engage in. The putative "intentionality module" may not have been (severely)

damaged in view of her "miraculous" subsequent recovery and participation in the human community by means of language (with the "intentional" producer and parser, two major performance modules, having access to FLN; see Suzuki 2006; also Rey 2003 for the "intentionality" problem in Chomsky's theorizing), with the consequence of the (putatively) damaged connecting channels between such intentional performance modules as the producer and parser, on the one hand, and FLN (possibly, plus other interfacing modules via FLN; see Suzuki 2006 for details), on the other, somehow having been restored after the pumphouse incident to the state in which biolinguistic resources were freely available for various language purposes. Gill's (1997) Argument 7 goes on to claim that " ... her (Keller's — *NS*) acquisition of language dramatizes the role of embodiment as well as that of speech in establishing the meaning of being human (emphasis — *NS*)." I would like to focus and speculate here a little bit on the possible relationship between the role of speech/-language and the property of being human from the viewpoint of Nakazawa's (2004) framework of symmetric anthropology (partly based on recent results from works of cognitive archeology), where efforts are made to derive possible differences between the mentality of the Neanderthals and that of *Homo sapiens* (i.e., from Cromagnon man downward). According to cognitive archeology (see Nakazawa 2004: 69-87), the Neanderthal brain used to be equipped with several modules, such as ones responsible for language, social matters, skills, and naturalistic things, with no connecting channels between or among them, leading to the assumption that symbolic thought was alien to it. In order for symbolic thought to become operative, the presence of neural channels cross-connecting distinct modules and the emergence of the "mobile intellect" that can move among them at a high speed must be indispensable. And it is the establishment of cross-connecting channels and the emergence of the mobile intellect that cognitive archeologists believe happened in the *Homo sapiens* brain. It should have been as if the main computer controlling the operation of other smaller computers (i.e., distinct modules designed for a specific purpose) had been established in the *Homo sapiens* brain, in addition to something like what the Neanderthal brain also possessed. Notice the striking similarities (with obvious reservations concerning the difference in the level of discussion, though) between the "main computer" in (early) *Homo sapiens* and our FLN, which serves the function of mediating between and among a number of interfacing modules by virtue of being available for their distinct purposes (i.e., SMT; see also Suzuki 2005, 2006). So it would surely be tremendously interesting and fascinating to compare what cognitive archeologists will have to say about the origin of the "main computer" above with the "evolutionary" interpretation of the SMT in Suzuki (2006: 41-45), some extended and generalized notion of the SMT presumably being appropriate for the description of the way(s) the main computer in the whole *Homo sapiens* brain operates and interacts with a variety of other distinct modules for a specific purpose, involving the explication of what the mobile intellect does by moving among the main computer plus these specific modules at a high speed. Chances are that it is only the whole *Homo sapiens* brain and the human language system with FLN in its

center that are governed and regulated by an organizational structure such as the SMT.

I would then like to comment on the observation in Argument 7 that the acquisition of language should constitute an important prerequisite for the establishment of the meaning of being human (ignoring possible problems arising from the notion of embodiment crucially used by Gill here again). There is no objection whatever on my part to the claim that language plays an indispensable part in the explication of what it is to be a human being. Taking issue with cognitive archeologists on the *Homo sapiens* mental structure, where distinct specific-purpose modules were able to cross-connect with one another via the "main, all-purpose computer" with the "mobile intellect" moving around among these modules at a high speed, Nakazawa (2004) goes on to claim that it may be necessary to introduce the "Unconscious" in the sense of Freud and Levi-Strauss into the *Homo sapiens* mind, the Unconscious (alone) being compatible with multi-/high-dimensional (roughly meaning "more than three-dimensional") structures and symmetric logic that has the potential to destroy Aristotelian logic. Further reformulating the concept of the mobile intellect in the sense of cognitive archeologists as the "mobile intellect with multi-/high-dimensional properties" in terms of his theory of symmetric anthropology, Nakazawa (2004: 74-76) arrives at the conclusion that the concept of the "mobile intellect with multi-/high-dimensional properties" must be equated with the notion of the "Unconscious," which Freud tried to distinguish from the Conscious. Then Nakazawa (2004: 78-81) goes so far as to claim that "the Unconscious constitutes language," the former being the mental phenomenon made possible only through the new method of connecting neurons realized in the *Homo sapiens* brain. The striking point here is that the Unconscious thus came to be able to engage as the multi-/high-dimensional mobile intellect in a variety of activities free from intellectual fields adapted to specific realities, with the consequence of the human being having succeeded at this time of human history in realizing the freedom from the outside physical world in the form of the establishment of the Unconscious system. Extremely interesting for our purposes is the long-standing observation that "the language that the child eventually acquires is somehow free from outside stimuli" (specifically presented as a counterargument to Skinnerian behaviorism). Some of the standard assumptions in regard to the normal child's factual acquisitional situation along the lines of Chomskian nativism go as follows (see, for example, Hornstein and Lightfoot 1981: 9-13): (i) the available data for acquisition purposes are finite, but the child comes to be able to deal with an infinite range of novel sentences, going far beyond the utterances actually heard during childhood (hence, free from the actual data/stimuli encountered during the acquisition process); and (ii) people attain knowledge of the structure of their language for which *no* evidence is available in the data to which they are exposed as children (leading to the idea of *absolute* freedom in the sense that human beings have access to resources that come from within themselves). Cook and Newson (1996) describe this free mental situation in regard to language in terms of the more popular term "creativity," as in the following: "In all Chomskyan models a characteristic of

competence is its creative aspect; the speaker's knowledge of language must be able to cope with sentences that it has never heard or produced before. E-language depends on history — pieces of language that happen to have been said in the past. I-language competence must deal with the speaker's ability to utter or comprehend sentences that have never been said before ...." (p.24); "Creativity in the Chomskyan sense is the mundane everyday ability to create and understand novel sentences according to the established knowledge in the mind — novelty within the constraints of the grammar. 'Creativity is predicated on a system of rules and forms, in part determined by intrinsic human capacities. Without such constraints, we have arbitrary and random behavior, not creative acts' (Chomsky 1975). It is not creativity in an artistic sense, which might well break the rules or create new rules, even if ultimately there may be some connection between them" (p.25; see Suzuki 2006: 45-47 for some discussion on and criticism of Chomsky's 2005b treatment of the notion of "deviance," and for the proposal for connecting indirectly to FLN the interfacing LPM module (responsible for the interpretation of literary, poetic, and metaphorical language) module, which kind of equipment pertaining to FLN may be readily available, given Suzuki's 2005, 2006 framework, where various interfacing modules (both competence-wise and performance-wise) can have ample, direct access to FLN in case they are responsible for local mental processes in the sense of Fodor 2000, other modules responsible for arguably non-local processes such as the one for which the LPM module above is responsible, for example, presumably being connected to FLN via some module related to them that interfaces directly with FLN, such as the normal LF module).

Nakazawa (2004: 78-79) goes on to give some typical examples that may show interesting close correspondences between language structure (due to Roman Jakobson), the Conscious system, and the Unconscious movement: (i) the syntagmatic relation in language structure, metonymy (as a Conscious resource), and replacement (as an Unconscious resource) correspond to each other, while (ii) the paradigmatic relation in language structure, metaphor (as a Conscious resource), and compression (as an Unconscious resource) correspond to each other. We have seen extremely close connection between the Unconscious and language or their virtual identity (see Nakazawa's 2004: 78 observation that the Unconscious constitutes language), both of which have considered here to constitute a clear defining property of *Homo sapiens*, as distinct from Neanderthal man, for example. This much should suffice to establish that language is by far the likeliest candidate to give substance to the meaning of being human. Retuning to the phrasing "phantom," Keller's living situation in those days must have been one where the damage resulting from the serious disease at the age of one year and nine months had the grave consequence of nullifying the various functions of the "main computer" and FLN, leaving her as one deprived of a variety of "humanly normal" emotions, intentions, perceptions, and, of course, language.

(19) < *Gill's (1997) problematic argument 8* >

"It is interesting and enlightening to think of sign language as a kind of onomatopoeia in which the sign and the signified are essentially united. Owen Barfield and others have argued that all language flows from such primordial unity of speaker, sound, and object (...). In Helen Keller's case this unity was intensely focused because of the proximity, indeed identity, among these three dimensions of human existence. Nevertheless, each and every child who learns to speak passes through essentially the same experience, though less dramatically" (p.52).

It may be quite interesting to reconsider and reanalyze Argument 8 above (which is not much of a problematic argument here, though) from the perspective of Suzuki (2006: 47-59), which is in part a drastic reinterpretation of the weakening in children of Principle B effects of the binding theory on the basis of Kiguchi and Thornton's (2004) important new data (pertaining to the problem of the presence vs. absence of sound in the portion of the sentence relevant to Principle B effects), with the consequence of proposing the following principle concerning the default interpretation of the phonology from the viewpoint of learnability:

(20) *Default interpretation of the function of the phonology*

All properties of the phonology affect semantic interpretation. (Suzuki 2006: 56)

Putative primordial onomatopoetic language with the *signifiant* and the the *signifié* essentially "united" (alluded to in Argument 8) may follow from (20) if the standard developmental/evolutionary scenario is on the right track according to which the child retraces throughout the various stages of her individual language development the wholesale development of language evolution involving stages even before the advent of FLN in the human organism. Problematic in the usual way, however, is the following portion of Argument 8 in (19): "In Helen Keller's case this unity (of speaker, tactile sign/sound, and object — *NS*) was intensely focused because of the proximity, indeed identity, among these three dimensions of human existence."

According to Helen Keller Campaigning Committee/*The Mainichi* (1948/2003), it was when she was ten years old that Keller uttered the sentence "It is warm today" in spoken English for the first time although the utterance was inaudible to herself. When she was seventeen, Keller entered a women's school in Cambridge to prepare to enter Harvard University. You had to take examinations in English, history, French, German, Latin and Greek classics, algebra, and geometry to enter Harvard University in those days. Sullivan attended classes with Keller, the former finger-spelling into the latter's hand what the teachers had to teach. When she had questions, Keller finger-spelled them into Sullivan's hand and the latter conveyed to the teachers in spoken language what Keller wanted to ask of them. After three years of inexpressible hardship, Keller succeeded in entering

Radcliffe College (attached to Harvard University), the first miraculous feat in world history, in the fall of the year in which she turned twenty-one. After four years of constant industry, Keller graduated from Radcliffe College with quite an excellent academic record. In October, 1904, when she graduated, the then ongoing St. Louis Exposition committee set up "Helen Keller's Day" in commemoration of her graduation and invited her to speak before the public (for the first time in her life). Keller talked in spoken English, but her voice was so small that the president of the Exposition, who happened to be close to her at that time, conveyed to the public in a loud voice what she had to say. Since there were still a number of problems with her method of vocalization, Keller repeated its study under the professor of vocal music at Boston Music School for three years from 1909. Starting with the first public lecture delivered in Mont Claire, Keller ended up giving several thousand lectures both in the States and in many other countries of the world for forty odd years afterward.

The term "unity" in the problematic portion of Argument 8 above should be understood to apply to the language situation in the early years of her life, when "primordial onomatopoetic language with the sign and the signified essentially united" can still be a viable option as her language. But the observation above in (19) only seems to contribute to the clearer explication of Keller's language situation around or after the pumphouse incident, when she must have been over seven years old. I presume that the principle of the default interpretation of the function of the phonology in (20), on the basis of which we can entertain the possibility of primordial onomatopoetic language in the early years of language development, may not have applied any longer to Keller at this time of her life. All in all, my hunch is that there was no such unity (of speaker, sound, and object) present in her mind/brain around or after the pumphouse incident, but that FLN must have been in place with its full consequences precisely due to that incident.

As for the actual situation surrounding Keller's language after the illness, my overall impression based on what Helen Keller Campaigning Committee/*The Mainichi* (1948/2003) has to say above is that while she continued to be endowed with innate human language abilities (FLN, etc.) as *competence* in the traditional sense, the serious disease that afflicted her at the age of one year and nine months continued to have the bad consequence of adversely affecting her *performance* abilities throughout her life afterward. Moreover, I would suspect that the only *natural modality* for Keller's language must have been the *tactile language by means of finger-spelling* (both processing-wise and production-wise; recall the scene where Keller and Sullivan finger-spelled into each other's hand in class) in view of her physical condition as one who was deaf, blind ,and mute, which situation with Keller's language should count as one of the most telling arguments in favor of the modality-independence of FLN. The predominance of spoken language seen in every aspect of her life simply shows how ignorant, indifferent, and ungenerous *ordinary* people are at any time in history.

(21) < *Gill's (1997) problematic argument 9* >
"The secret lies, it seems, in our ability to grasp and incorporate two activities in one, to participate in and thus understand one process in and through another" (p.55).

Pointing out the apparent impossibility of teaching children the meanings of words by giving their ostensive definitions and also the seemingly quite difficult task of enabling prelinguistic children to participate in language, Gill (1997: 54-55) suddenly arrives at the conclusion that while not susceptible to intellectual explanation, the mystery is resolved quite well at the activity level, adding the observation above in (10) to this. Gill goes on to try to show that the process involved should be an excellent paradigm of the larger question concerning the acquisition of language in general by equating the logic involved in the situation where we are observed to do two things at once, namely, pointing at an object or place and showing how the word is used when trying to teach such relative words as "here," "there," "this," and "that," with the logic pertaining to the acquisitional situation in which children imitate adults *at one level* and thereby come to tparticipate with them *at another level* of speech/language (p.55; emphasis — *NS*).

Despite Gill's (1997: 55) emphatic repetition of the hypothesis that "the sort of level of knowledge involved in language acquisition is clearly not to be understood as separate from each other," I suspect that his position is no longer sustainable. I show this in terms of the extremely important and interesting consequences and theoretical implications for Chomskian nativism that arise from Nakazawa's (2004) theory of symmetric anthropology. In marked contrast to the cognitive archeological observation that the Neanderthals may largely have been able to do only "naïve" things due to their mental system where a number of specific-purpose modules existed in large measure in a manner separate from each other, *Homo sapiens* came to produce symbolic thought with the mobile intellect in place to move around among these specific-purpose modules. Apart from the putative "primordial" stages of language development when "the sign and the signified are essentially united," language with its so-called "symbolic" property as its functional core must involve the language module and one or more other specific-purpose modules to operate in a meaningful way as such. So a number of factual assumptions in regard to language point to the implausibility of Gill's (strong) claim that every level or piece of behavior or knowledge concerning language must be understood as residing in the single place of what he calls the "body" (as distinct from the "mind" in the sense of traditional dualism). As for the implications of the resulting discussion here for the observation of Argument 9 above in (21), "doing two things at a time" must involve connection of some sort between at least two distinct specific-purpose mental modules, a mental matter par excellence, in sharp contrast to the implicit assumption in Argument 9 that the two things/activities in question should be somatic in nature.

4 Symmetry, Asymmetry/Antisymmetry, and Related Matters

4.1 The Unconscious and Language

Some comments on Nakazawa's (2004: 78) observation that the Unconscious constitutes language may be in order here. We have seen above that the multi-/high-dimensional mobile intellect appearing in the *Homo sapiens* brain that moves around according to symmetric logic among various specific-purpose modules/components with the all-purpose computer at the core of the human brain system may be equated with the Unconscious in the sense of Freud. Then a more precise wording of his (2004: 78) observation may be that "the Unconscious constitutes the function of language" or that "language functions in the Unconscious fashion." (Notice, however, that Reuland 2001 argues that various types of anaphoric dependencies form an economy hierarchy in such a way that a more costly dependency is disallowed provided a cheaper alternative is available. According to him 2001: 472, "... the operations of narrow syntax are subliminal. They are automatic, hence, plausibly, cheap. Computations within the interpretive component may well be generally more costly. ... the task of establishing a referent for some pronoun on the basis of preceding context requires processes that involve conscious access to various data structures, and hence may be slowed by nonlinguistic factors." So it may follow from this that strictly speaking, only narrow syntactic computation functions in the Unconscious fashion. I presume that "cheap" operations must be "easy" ones. They are subliminal/subconscious/-Unconscious. Being easy should point to the situation where the current linguistic gestalt must be congenial to being human; see also Avrutin 2006 and Suzuki 2006.) And, of course, an intriguing possibility that remains to be explored in future research should be the equation of the "main computer" in the *Homo sapiens* brain with FLN, a completely new map of the human brain system. But chances are that only half of the *Homo sapiens* brain (i.e., the "main computer"-related portion) could be equated with FLN, largely thanks to properties it shares with language, such as locality, modularity, domain-specificity, and innateness. The obvious dichotomy in human mental process type in terms of Alan Turing's computational theory is asserted by Fodor (2000: 5) to be between mental processes that are local and ones that are not, namely global. While much knowledge of the local type of mentality has been accumulated, we do not know much about global cognition or deeply understand it. My speculation here is that *local* cognition/language was precisely what *Homo sapiens* alone acquired roughly 200,000 years ago, while *global* cognition must be the older type of mentality that *Homo sapiens* shared with the Neanderthals.

4.2 *Nakazawa (2004) and Moro (2004): Preliminary Discussion from the Perspective of "Compression"*

Exploring further consequences arising from the striking similarities in structure between the *Homo sapiens* mental/brain system (specifically, as it pertains to local processes; see Fodor 2000: 5) and the human language system with FLN as its core, I would presume that the crucial presence and employment of the term "compression" both in Nakazawa (2004) and in Moro (2004) may not be just a mere coincidence. It is pointed out in Nakazawa (2004: 73) that "Compressed or replacing expressions are sure to arise if what the multi-/high-dimensional Unconscious tries to convey is to be adapted to the logic of the real, three-dimensional world. It then follows not that metonymy/metaphor and symbolization derive from combinations of pieces of logic of the three-dimensional world, but that metonymy/metaphor and symbolization treated in terms of compression and replacement arise every time the multi-/high-dimensional Unconscious encounters the three-dimensional logic grid," while Moro (2004: 395), which is a weak version of Kayne's (1994) theory of the antisymmetry of syntax in the sense that it allows pre-movement LCA-incompatible structures to be created by Merge[2] (leading to the economy-based operation of the LCA, as opposed to Kayne's 1994 original system requiring the LCA to hold at all levels of representation and thus prohibiting the generation of points of symmetry anywhere; with her framework dubbed *dynamic antisymmetry;* see also Moro 2000) and an attempt to present a dynamic antisymmetric theory of movement in the general framework of Chomsky's Minimalist Program as an alternative to the more or less standard theory of movement with morphology (in the form of uninterpretable features) as a trigger for movement, observes that "... movement can just be regarded as a way to rescue the structure at the interface with phonological component in case a point of symmetry (i.e., a structure too symmetrical to be linearized in terms of the LCA — *NS*) has been generated: movement deletes the phonological features of one element constituting a point of symmetry and copies it in a suitable position, thus solving the problem linearization at PF. Technically, let us say that movement 'neutralizes' a point of symmetry. To put it more generally, *movement is regarded here as a consequence of the physical necessity to organize words into a linear order* (*call it 'linear compression'* ; emphasis — *NS*) as opposed to the standard theory which considers it as triggered by uninterpretable features. ... the crucial role of linearization (which again is only due to a property of the organic-physical world, rather than conceptual necessity) in the explanation suggests that *a specific syntactic property may be related to the conditions imposed on the language faculty by the external world* (emphasis — *NS*), as originally envisaged by Chomsky (2004). ... *an aspect of the physical world, i.e. the fact that structures must be flattened to produce utterances* (emphasis — *NS;* see also Nunes 2004 and Fox and Pesetsky 2005 for an approach to the linearization problem with special emphasis on consequences of the LCA for chain properties and another to it from the viewpoint of a version of cyclic linearization that gives an interesting

means of understanding the reasons for successive-cyclic movement, respectively)."

Presenting somewhat detailed analytical consequences of her (2000, 2004) dynamic antisymmetry framework (specifically involving the "symmetry-breaking" nature of movement) mainly in the two empirical domains of the type of constructions involving the notion of "small clause" which should be understood as that of "bare small clause" (as opposed to "rich small clauses," which are projected by a head and serve as complements of *believe*-type verbs, for example; with "bare small clauses" functioning as complements of functional heads such as T^0 (the copula), of D^0, and of C^0, giving at the same time the reason why either the subject or the predicative noun phrase must raise to neutralize a point of symmetry; see also Moro 1997), and of the *wh*-movement case, where the relevant point of symmetry is *internal* to the *wh*-object and the way the point of symmetry is neutralized forces further movement involving a form of pied-piping (see the typology of *wh*-movement both across and within languages), Moro (2004: 412-415) goes on specifically from the perspective of the possible explanation as to what *triggers* movement to discuss some theoretical consequences of Chomsky's (2004) theory of movement, which roughly is grounded on the following two major assumptions: (i) movement is internal Merge; and (ii) movement is triggered by the necessity to delete uninterpretable features.

As there seems to me to be a certain gap between Moro (2004) and myself in understanding some consequences of Chomsky's (2004) theory of movement in spite of my intention here to explore possibly positive aspects of Moro's (2004) dynamic antisymmetric theory of movement, according to which "linear compression is a trigger for movement," let us first look at the following portion(s) in Chomsky (2004) that Moro (2004) cites as problematic (the latter depending for her information and argument on Chomsky 2001: Beyond explanatory adequacy, *MITOPL* 20, Cambridge, Mass., though):

(22) "What about internal Merge? We expect its application to be motivated by the *nontheta-theoretic C-I conditions: scopal and discourse-related (informational) properties in particular* (emphasis — *NS*). ... Hence H has OCC only if that yields new scopal or discourse-related properties .... No nonlocal or look-ahead conditions are introduced. If H has OCC, then the new interpretive options are established if OCC is checked by internal Merge; it is only necessary that the cyclic derivation D can continue so that they are ultimately satisfied with convergence of D. *Informally, we can think of OCC as having the "function" of providing new interpretations; in the analysis of any process or action (the operation of the kidney, organizing motor action, generating expressions, etc.) such functional accounts are eliminated in terms of mechanisms* (emphasis — *NS*)" (Chomsky 2004: 112-113).[3]

Roughly, the gist of Moro's (2004: 412-414) critical remarks on Chomsky's (2004) theory of movement (i.e., internal Merge), briefly reported in (22) above, is that she seems to suspect that Chomsky's is a "functional" theory of movement. But note that Chomsky (2004)

himself notices the seeming functional aspect of his system of internal Merge, apparently explaining away the deficit by resorting to the organizational nature of the mechanism involved (see (11)). I would claim that the circumstances in question are not a deficit at all, the notion of the SMT playing a major role here in showing Chomsky's (2004) system to be preferable, in fact. Look at the following two versions of the SMT (as it applies to the C-I interface):

(23) a. Language/FLN is an optimal solution to legibility conditions at the C-I interface. (à la Chomsky 2000a)

b. Every piece of innate linguistic/FLN knowledge reflects some innate knowledge in the C-I domain. (à la Suzuki 2005)

Both versions should be understood from the viewpoint of the evolutionary interpretation of the SMT in the sense of Suzuki (2006). Overall, the SMT points to the wholesale organizational nature of the entire human organic system that is designed to exclude in principle "functional" theorizing, "nonlocal" or "look-ahead" properties, and the like.

While I plan in my forthcoming work to turn to the more detailed discussion of Moro's (2004) extremely interesting dynamic antisymmetric theory of movement crucially based on the *cross-dimensional* concept of compression, which is somewhat beyond the scope of the present paper at the moment, let us nonetheless review some of the most representative approaches to possible conditions on the correspondence between FLN and various interfacing modules/systems with some discussion on each of them.

4.3 The Parser, the Top-down Approach to Structure-Building, and FLN

Reinhart (2004, 2006) argues on the basis of much empirical evidence for the necessity of strengthening the grammatical system by the additional means of what she calls *interface strategies* (for the purposes of repairing imperfections), which are supposed to have the function of compensating for inadequacies arising from "imperfect" FLN (according to her 2006: 6-8, in spite of Chomsky's constant insistence on its perfection; Chomsky 2000a, for example) failing to achieve a complete correspondence between the requirements on the part of interfacing systems (SM, C-I, etc.) and the resources it can provide. While admitting that "language is *optimally* designed, though it is not *perfect* (p.8; emphasis — *NS*), Reinhart (2006: 7) takes issue with Chomsky (2000a: 96) over the degree of perfection of FLN in connection with empirical conditions such as acquisition, processing, neurology, etc.,as well as various interfacing modules, and is quite explicit that "what is outlined here (Chomsky 2000a: 96 — *NS*) is the (unrealistic) perfect solution (rather than the optimal)." Reinhart's (2006) observation here is quite important in that it explicitly questions the validity of Chomsky's SMT as formulated in Chomsky (2000a: 96), which version of the

SMT the present paper adopts as such, the gist of Reinhart's (2006) expression of doubt being that *Chomsky's (2000a: 96) SMT may be too strong.* (I henceforth call Reinhart's 2004, 2006 system the "weak" version of the SMT.)

Reinhart (2006: 7-12) gives some empirical evidence in connection with the parser-FLN/CS (i.e., the computational system) correspondence in favor of her "weak" version of the SMT. While Phillips's (1996) hypothesis that "the parser is the grammar" seems to adopt the strongest possible version of a "transparent parser" (with the question of how "transparent" the parser can be interpreted as the problem to what extent it can directly apply computations of the FLN/CS rather than its own independent algorithms), Reinhart (2006) takes up much-discussed problems with what are called "garden path" sentences, such as the following (taken from Reinhart 2006 and Mulders 2005):

(24) a. Max knew Lucie would laugh.
b. Max warned Lucie would laugh. (garden path)
c. After Susan drank the water evaporated. (garden path)

The standard assumption is that the "garden path" property usually arises out of local ambiguity at a processing stage (such problems never arising in syntax). Note that Mulders (2005) is an interesting attempt to distinguish between non-garden-path sentences with local ambiguity, such as (24a), and garden-path ones with local ambiguity, such as (24b,c), in terms of the processing constraint in (25) reformulated on the basis of Chomsky's (2001) phase impenetrability condition (PIC):

(25) *Phasal Constraint on Reanalysis* (*PCR;* first version)
In case of reanalysis from source position S to target position T:
if S is phase-embedded with respect to T, S must be in the edge of a phase.

Speaking of the "reanalysis component," Mulders (2005: 240) emphasizes that "our requirement for the parser to be transparent implies that the constraint on reanalysis should be on a par with constraints on movement in syntax." Quite close to the CS/FLN as Mulder's (2005) processing system appears to be, the very necessity of the "reanalysis component," which is not needed in any way for syntactic purposes, must be a telling piece of evidence that it cannot be said that the parser and the CS/FLN are one and the same in Mulders's (2005) system, which may be another instance with "weak" SMT properties specifically in the case of the parser-FLN correspondence. Since the present paper keeps to the standard (see Chomsky 2000a, 2001, 2004) or (preferably) stronger (see Frampton and Gutmann 2002) version of the SMT, this problem of the apparent gap (albeit small, presumably) between the parser and the CS/FLN should be properly addressed, preferably in such a way as to approach a more profound understanding the true nature of the SMT as interpreted from the perspective of evolution (see Suzuki 2006).[4]

Confronted with the "garden path" problem with Phillips's (1996) very strong hypothesis that "the parser is the grammar," we may assume based on Suzuki (2006) that the only biolinguistically/ontologically feasible grammar is one with top-down derivation (see also Terada 1999, 2002). This is a performance-based grammar of the sort that has been proposed and described in Suzuki (2006: 59-67), in part as follows: FLN/SG (supergrammar, in the sense of Fodor 1998) and other ingredients (for the purposes of grammar formation) are just raw materials to be accessed and employed on the part of such performance modules as the parser and the producer, the resulting grammars (just epiphenomena, though; see Suzuki 2006: 67) being necessarily top-down in derivation due to the very nature of the performance modules involved. Suzuki (2006: 67) notes that if this line of reasoning is on the right track, it would call into question the factual basis of the standardly assumed grammar that proceeds bottom up in its derivation. But notice that pronunciation and signing must be linearized (i.e., "one-dimensional," according to some researchers) to be meaningful as possible means of linguistic expressions in this world. Then it may follow that performance-based grammars should be one-dimensional *right from the start* thanks to the one-dimensional nature of the relevant performance modules, specifically in the domain of linearization. (Even performance-based grammars may penetrate into dimensions other than the first one for various grammatical purposes, linearization aside.) While the domains of one-dimensional pronunciation and signing have to interact directly with the physicality of the world, concretely and specifically, with the property of "time," which seems to necessarily be one-dimensional, it remains to empirically ascertain what number *dimension* the properties of other components (e.g., syntax, interpretation) may belong in (presumably, the decision being subject to economy considerations in regard to the choice of a dimension).[5]

I would here like to present some arguments largely in favor of the top-down approach to FLN structure-building, as argued in Suzuki (2006; see also Phillips 1996, 2003, Terada 1999, 2002). My central assumption in regard to a (particular) grammar is that it arises as an epiphenomenon as a result of the parser or the producer accessing and capitalizing on what Suzuki (2006) calls "raw materials," which comprises FLN/SG ingredients and ones from other modules, these raw materials for (epiphenomenal) grammar formation (e.g., innately specified parameters with their two options constantly in place due to SG, but with a special bias for the options realizing the relevant properties of the parser's or producer's target or particular language) being the only biolinguistically, physically real entities, which may presumably reside in a number of different places and modules (and, perhaps, in different dimensions). Before getting on to the gist of our discussion, let us once again identify the exact structure and empirical extent of FLN. I largely follow Chomsky (2004), Hauser, Chomsky, and Fitch (2002), and Fitch, Hauser, and Chomsky (2005) in this attempt. Fitch, Hauser, and Chomsky (2005: 182) cite the observation from Hauser, Chomsky, and Fitch (2002: 1573) as a proposal for delimiting FLN: "We propose in this hypothesis that FLN comprises only the core computational mechanisms of recursion

as they appear in narrow syntax and the mapping to the interfaces" (emphasis — *NS*). Specifically, they (2005: 182) go on to stress that "... a significant piece of the linguistic machinery entails recursive operations, and that these recursive operations must interface with SM (the sensory-motor system; phonetics/phonology — *NS*) and CI (the conceptual-intentional system; semantics/pragmatics — *NS*) (and thus include *aspects of phonology, formal semantics and the lexicon* insofar as they satisfy the uniqueness condition of FLN, as defined (emphasis — *NS*; "uniqueness" defined for *Homo sapiens* — *NS*)). In terms of Chomsky (2004), the mapping components to the LF- and PF-interfaces correspond to the semantic component Σ and the phonological component Φ, which are crucially part of FLN. (Note that the latter components belonging under FLN that map narrow syntactic derivation to SEM and PHON, which should correspond to the LF- and PF-interfaces, must be instrumental in making FLN itself usable on the part of the CI and SM systems, thus contributing toward understanding the concrete way (part of) the SMT can be feasible.) This picture of the possible connection between FLN and systems/modules external to it (but internal to the human organism) by means of a related component internal to FLN and an interface (accessed by an FLN-external system) as a result of that component mapping more FLN-internal derivation to it would have rather recalcitrant consequences that may cause a lot of trouble and worry to such a framework as Suzuki (2006), where any system external to FLN that is local, innate, domain-specific, and modular (in the sense of Fodor 2000) can have access to FLN to interact with it for some specific purpose. But as we have seen in Chomsky's (2004) influential exposition above, the mechanical properties of FLN may require part of it to be an independent component related to the relevant FLN-external system that is especially designed for the related grammatical/linguistic purposes (i.e., a specific instance of the implementation of the SMT in regard to legibility conditions at interfaces). Among the problems here with Suzuki's (2006) framework are one of whether or not the similar requirement is imposed on other FLN-external modules trying to access FLN and another of how to procure the required mechanism if needed. Systems of nonlocal cognition might try to reach FLN indirectly via local modules that can arguably have access to it. The more difficult problem should be one of how to procure an FLN-internal component for systems of local cognition. Pending future research, I close the discussion for now on the empirical nature and content of the "mapping to the interfaces" component by citing the following from Fitch, Hauser, and Chomsky (2005: 182): "These mappings themselves could be complex (though we do not know) because of conditions imposed by the interfaces. But our hypothesis focuses on a known property of human language that provides its most powerful and unusual signature: discrete infinity."

In view of the discussion on the empirical extent of FLN, the formation of "on-line" bi-directional channels/passage at the completion of each strong phase (in the sense of Suzuki 2006) must basically take place within the bounds of FLN. But consider, for example, the "LPM" (based on "literary, poetic, and metaphorical language") module proposed

by Suzuki (2006: 47) in connection with the discussion on how to interpret what Chomsky (2005b) calls "deviant" expressions (see Suzuki 2006: 45-47). While Chomsky (2005b) does not appear to take into consideration the status of non-normal expressions in terms of an economy hierarchy governing the computational load involved in generating expressions (see Reuland 2001, for example), just observing that "Merge can apply freely, yielding expressions interpreted at the interface in many different kinds of ways. They are sometimes called 'deviant,' ... And expressions that are 'deviant' are not only often quite normal but even the best way to express some thought; metaphors, to take a standard example, ... That includes even expressions that crash (emphasis — *NS*), often used as literary devices ... The only empirical requirement is that SM and C-I assign the interpretations that the expression actually has, including many varieties of 'deviance'" (Chomsky 2005b: 10). Pointing out that Chomsky's (2005b) discussion above on "deviant" expressions is too crude a characterization of the precise mechanism for the interpretation of such things as literary expressions, Suzuki (2006) argues for the necessity of finer analytical distinctions between ordinary and literary expressions and proposes that the actual differences between the two cases should be located in the specific way that the module responsible for each kind of expressions may connect to FLN (directly or indirectly, for example) during the process of sentential derivation. He (2006: 47) then proposes an FLN-external (but internal to the organism) module/system called the "LPM" module that is responsible for the interpretation of literary, poetic, metaphorical (, etc.) expressions, which can only connect to FLN indirectly via the LF module, which I assume to be responsible for the interpretation of ordinary, normal expressions. Chances are that the LPM module may not have anything like the FLN-internal component for its purposes (similar to the semantic component Σ for the purposes of the LF module), but my suspicion is that the LPM module may have an LF module-internal component for its purposes and that the LF module may have ingredients in it that can serve as interface to bridge the LPM module and itself, the LF module presumably possessing *properties that satisfy LPM-related interface conditions*. The workings of the LF module toward the LPM one described in some detail in Suzuki (2006) and only briefly here, with the concomitant presence of an interface intervening between the two modules, might amount to an instance of some "generalized" version of the SMT in the sense that in the terminology calling FLN the "host" and interfacing systems such as SM and CI the "guests," it could apply to a host-guest pair (i.e., the pair comprising LF- and LPM-systems, in the case under discussion) other than the standard host-guest pair consisting of FLN and familiar interfacing systems, with usual interfaces bridging the two, as has originally been formulated by Chomsky (2000, 2001, 2004). I would like to further speculate on what the design of the human mind is, specifically from the biolinguistic, evolutionary perspective of the way the SMT is presumed to operate. In the terminology where FLN and interfacing systems correspond to the host (or the "main computer") and the guests (or the "child computers"), respectively, the elementary properties of the guests are (presumably) biologically and

innately "copied" into the relevant part of the host, with the concomitant presence/emergence of interfaces bridging the two parties. The human brain might perhaps be an intricately embedded collection of such host-guest pairs, whose function and workings are constrained by the SMT. If Nakazawa (2004) is on the right track when he claims that the Unconscious in the sense of Freud must be language (i.e., FLN, I assert) and, presumably, if the Unconscious/FLN alone has the potential to go beyond the third dimension (see section 5 below) crucially under the assumption that the presence of the Unconscious/FLN in it is the sole defining property of the *Homo sapiens* brain distinguishing it from that of the Neanderthals, for example, FLN should presumably be situated atop of the whole complex of such multiply embedded host-guest pairs, commanding the entire brain system crucially in the SMT fashion (i.e., something like the "SMT-based architecture of (part of) the mind/brain"; apart from portions of the system that are governed by nonlocal/global principles in the sense of Fodor 2000).[6]

As for the status of performance modules such as the parser and the producer, I here simply follow Suzuki (2006) in assuming that they are local mental processes, being computational, modular, and innately specified, and hence that they can connect directly to FLN. But as you see, there immediately arise at least two major problems with this assumption: (i) What FLN-internal ingredient(s) may these performance modules have in the fashion of the PF- and LF-modules, for example?; and (ii) How may the long-standing recalcitrant concept of "intentionality" come into play and interact with these modules? I address these problems only briefly below, given the current level of understanding of the relevant domains.

As for the problem in (i), what I call FLN-internal ingredients roughly corresponds to the semantic component (Σ) and the phonological component (Φ) in the cases of the CI and the SM systems/modules, respectively, thus serving as the mapping mechanism to the interfaces, SEM and PHON, respectively. Since the most parsimonious and hence interesting model of FLN may contain as its only resources the core computational system, comprising the operation of Merge and the property of recursion, and the mapping to the interfaces (see Fitch, Hauser, and Chomsky 2005: 182), the latter presumably being needed to bridge a gap between purely narrow syntactic products and the resultant interfaces, the latter of which are supposed to consist of representations satisfying requirements on the part of the relevant, external (to FLN) systems/modules, it should be clear and plausible, *given the SMT*, that the performance systems, such as the parser and the producer, should have something internal to FLN in the service of connecting FLN with the interfaces relevant to them, and it should also be the case that such FLN-internal resources pertaining to these performance modules must be innately specified. Roughly the same must be true at least of several other external systems, such as the pragmatics, the lexicon, and so on, which Suzuki (2005, 2006) assumes connect directly with FLN. (Perhaps, one or other of Chomsky's notions of "numeration" and "lexical (sub-)array" may serve as the mapping to the interface in the specific case of the lexicon, under the assumption that it connects

directly with FLN.) In the absence of enough research and evidence bearing on the issue, I simply have to await future work in this direction, noting in this connection that such enrichment of the mapping mechanism to the interfaces (which is somehow forced by empirical considerations) does not necessarily go against Chomsky's (2006) intention of reducing to the minimum the amount of purely narrow syntactic ingredients, which cannot possibly be attributed via the SMT to any properties deriving from external systems.

I turn now to the long-standing recalcitrant issue of "intentionality." It may be possible to discard the problem by claiming that the relevant framework is only concerned with something like "mathematical proofs" when constructing sentences and hence, no issue of intentionality can arise. But we have a fairly long history of the problem of reducing the computational load in the very domain of narrow syntactic derivation. If computation is like mathematical proof, why can the size of its load be a problem? But, conversely, the very idea of a load surely arises in the context of human *endeavor* (whether unconscious or conscious). As I have discussed in some detail in Suzuki (2005, 2006), it seems to be the case that the traditional distinction between (linguistic) competence and performance (see Chomsky 1965) has somehow come to lose its force, in favor of the assumption that the relevant, biolinguistically significant distinction should be made between the unconscious and the conscious. Then I would reinterpret the computational load of the sort arising in the course of narrow syntactic derivation largely as an unconscious load of some sort, which must nevertheless adversely affect the language producer simply because it is a load. As for the issue of intentionality, then, I do not discard it by saying that since computation is like mathematical proof, the problem of intentionality never arises. Rather, I take the issue to be one of the most important problems to be addressed in the current context of biolinguistic research. But the reality surrounding the concept of intentionality may not be as remarkable or as impressive as one might expect. I basically follow Maeno (2006) in the interpretation of "free will," in assuming that "the contents of the mind, the consciousness, and free will have been created as a result of the interaction of neural cells with each other in the brain, which are physical matters. Maeno's (2006) basic position is that "both physical and mental phenomena are objectively observable." It follows then that "the consciousness and free will are simply virtual fantasy without any factual existence, it being impossible for the consciousness or free will to be other than matter" (Maeno 2006: 100). While Maeno's (2006) theorizing seems to basically be based on some version of materialistic monism, I believe in large measure that he is on the right track. Then roughly, equating the concept of free will with that of intentionality, I would take the latter to an epiphenomenon crucially arising in the course of the interaction of neural cells with each other belonging in the language domain(s) in the entire brain and, naturally, in brain domains other than language (perhaps, with the issue of how to connect linguistic with non-linguistic domains pertinent to the concerns of current neurology). Moreover, I would like to claim that possible transportation via the

bi-directional passage method between FLN and other components/systems in the framework of Suzuki's (2005, 2006) SMH that allow ample bi-directional access to a number of interface components/modules in the course of narrow syntactic derivation may partially contribute to the creation of a situation involving neural cells of the relevant sort in the brain, where something like virtual fantasy resembling an instance of intention may arise. And I would take Chomsky's notions of numeration and/or lexical (sub-)array (insofar as they are assumed to be valid in linguistic theorizing) to be evidence of the most transparent sort for the presence of intentionality, albeit as an epiphenomenon as noted above.

4.4 Sigurðsson (2006a): Low Nominative Hypothesis

I would like here to discuss in some detail Sigurðsson's (2006a) "low nominative hypothesis" in such a way that it may count as an empirical piece of support in favor of Suzuki's (2006) top-down approach to narrow syntactic derivation (see also Phillips 1996, 2003; Terada 1999, 2002), making an attempt at the same time to introduce (some of) the core ingredients of Sigurðsson's (2006a,b) framework so long as they are indispensable to our discussion here and somehow trying to refute his (2006a: 292) observation in his note 3 to the effect that a top-to-bottom approach fares no better than a bottom-to-top one with respect to (part of) the state of affairs that he calls the *Nominative Puzzle*. First look at the following pair of formulations of Burzio's Generalization (BG):

(26) a. All and only the verbs that can assign a θ-role to the subject can assign accusative Case to an object. (Burzio 1986: 178)
b. All and only the verbs that take a *Nom* subject can assign structural Acc to an object. (Sigurðsson's 2006a (7))

Sigurðsson (2006a: 290) somehow reformulates Burzio's (1986) original, more or less standard formulation of BG in (26a) as (26b), based on Sigurðsson (1991), which argues that "nominative Case is active in PRO infinitives," and hence on the resulting view of BG as a correlation between the *structural Cases*, rather than between the external role and the internal Case. It is immediately clear that it follows from (26b) that structural accusatives are contingent on structural nominatives, leading to the proposal of what he calls the *Sibling Correlation* (Acc being the "younger, dependent sibling"; Sigurðsson 2006a: 290). Sigurðsson (2006a: 291) further observes that "the dependency correlation between structural Acc and Nom is a striking phenomenon that holds across (the core of) accusative s ystems," adding that "even in languages like Icelandic and German that have some quirky accusatives, most accusatives are structural, hence only possible in the presence of a nominative subject." He (2006a: 292) goes on to point out that given the Sibling Correlation, the standard minimalist bottom-up approach to narrow syntactic derivation, where

Acc is the first Case, merged lower than Nom, is immediately faced with a peculiar problem, which situation he calls the Nominative Puzzle (including other consequences as well of adopting the Sibling Correlation). The standard Case-theoretic assumption has been that nominative is merged *after* accusative, being the marked or complex Case. Sigurðsson (2006a) claims, however, that the actual fact is the other way around, "nominative preconditioning accusative and also being the Case of simple structures (unaccusative, unergative, and, in many languages, predicative)."

The major part of Sigurðsson (2006a) can be considered to be an attempt to establish that the Nominative Puzzle is not real, "the Nom argument actually being merged as the first argument, raising across Acc later in the derivation for independent reasons," breaking at the same time the T-Nom connection (to the effect that finite Tense alone assigns nominative (under agreement)) and thus solving many recalcitrant problems arising from the existence of "nominative objects and nominative predicative DPs" in various kinds of infinitives in Icelandic, of "overt, contrastively focused nominative subjects" in colloquial Icelandic negative infinitives, and of "low nominatives" in German and Swedish predicative constructions. Keeping to the strictly "dumb" computational system with no "look-ahead" ability specifically during the process of more than one instance of Case assignment, Sigurðsson (2006a: 294) goes on to explore the "obvious" solution to the Nominative Puzzle, namely, that it is not real, nominative in fact being the lowest or first Case, merged before any other Cases, as a sister of a predicate, which suggestion he refers to as the *Low Nominative Hypothesis* (LNH).

The assumption that nominative is the first Case to be merged is quite impressive from our viewpoint endorsing some version of a top-down approach to syntactic derivation, but Sigurðsson (2006a: note 3) observes at the same time that "This problem (arising from the incompatibility of the standard view that derivation operates bottom to top, which should force us to assume that accusative is the first Case to be merged, with his (2006a) *Sibling Correlation* to the effect that structural accusatives are contingent on structural nominatives, the latter entering the derivation earlier than the former — *NS*) might seem to be avoidable in a top-to-bottom approach (...), but in fact such an approach fares no better than a bottom-to-top approach. Thus, in a German clause beginning with a DP like *Peter*, the derivation would have to look ahead 'downward,' as it were, in order to know if the DP is an experiencer dative, an inherent accusative, a nominative." While we could come up with some hopefully plausible grammatical/parsing devices to cope with the German example in Sigurðsson's (2006a) note 3 above so as to save a top-down approach, I would like to submit the problem and Sigurðsson's (2006a) intriguing LNH, more generally, to further research, noting in this connection the great importance and relevance to our concerns here of Phillips (2003), where several of the major constructions pertaining to (comparative) ellipsis, scope effects (involving QR), and so on, are discussed and analyzed in terms of a top-down approach to structure building with special emphasis on the constituency of sentential elements, the key principle being his (2003: 37)

Incrementality Hypothesis, which states that "sentence structures are built incrementally from left to right."

4.5 Reinhart (2006): Reference-Set Computation

Consider the operation of "stress-shift" as a PF-interface strategy (see Reinhart 2004; 2006: 238-259). Roughly, the neutral main stress rule (originating from Chomsky and Halle's 1968 Nuclear Stress Rule; called the NSR, henceforth), which is an obligatory rule of the phonological component Φ (part of the "mapping to the interfaces" component of FLN; see Fitch, Hauser, and Chomsky 2005), applies to produce the focus set of the sentence in question consisting of the constituents containing the main stress of TP, the unmarked situation being one in which one member of the focus set being selected as the actual focus of the sentence at the SM-/PF-interface/PHON. But a problem arises in the case of examples such as "My NEIGHBOR is building a desk" (with the focused/stressed element CAPITALIZED), where the desired focus ("NEIGHBOR") does not count as a member of the focus set produced by the NSR for this sentence. A "stress-shift" operation, a PF-interface repair strategy, then applies in such a case to produce the sentence with the desired focus, a costly result (according to Reinhart 2006; see also Reinhart 2004, 2006 for much detailed exposition of stress-shift). In addition to it counting as a weak version of the SMT, the further problems with Reinhart's (2004, 2006) interface strategies framework may first include that of finding a natural place in the grammatical system for these interface repair strategies. Since the PF-interface/PHON is a combination of levels cyclically produced in parallel with the cyclic derivation of NS and the semantic component Σ, stress-shift must have applied in the phonological component Φ to show its result at the PF-interface/PHON in the form of the presence of the desired focus in the sentence. Then, as the phonological component Φ is part of FLN, it should follow that stress-shift is innate. This situation would entail ranking of some sort of innate principles/rules belonging to the same component in markedness hierarchy, such as "unmarked" NSR vs. "marked" stress-shift, not a very desirable consequence (despite Reinhart's 2006: 1 observation that reference-set computation is much more restricted than assumed in Optimality Theory). One further possible problem with Reinhart's (2004, 2006) system concerns her very general key concept of "reference-set computation." While it was already abandoned as a grammatical device pertaining to narrow syntax in early minimalism days, the mechanism of reference-set computation, which is the construction of a (global) comparison set to determine whether a given derivation is appropriate in context and comes with a processing cost, is argued by Reinhart (2004, 2006) to nevertheless be available to the *computational system*, as witnessed in some areas of the interface, applying as a "last resort" only when the outputs of core syntax/FLN operations are insufficient for the interface. Although the coverage on the part of Reinhart's (2004, 2006) notion of reference-

set computation is remarkable, ranging over processing, acquisition, and so forth, I suspect that the human computational system may not allow any such comparison of (more than one) derivations. (My alternative would roughly be to capitalize on the framework of Suzuki 2005, 2006, where the parser and the producer can have economy-based access to FLN and other interfacing modules via FLN through the bi-directional channels created between/among such components/modules at the completion of each strong phase during the process of sentential derivation. Presumably, high cost and slow speed in the cases observed may be attributed to the parser/producer having to have access to a larger number of components/modules for some grammatical purposes. I await future research along the lines sketched here for further depth and extension of the framework in coverage and empirical evidence.)

On the interpretation side, Reinhart (2006: 101-110) speaks of a LF-interface repair strategy in terms of "scope-shift" by means of QR. Reinhart (2006: 102) starts by comparing the version of an economy principle on the basis of the notion of "least effort" largely in the tradition of Chomsky (1995) with her (2006) version based on the concept of "minimizing interpretive options."

(27) *Least Effort Principle*
"If a derivation D converges without application of some operation, then that application is disallowed" (Chomsky 1992. *A minimalist program for linguistic theory*. Cambridge, Mass.: MITWPL: 47)

(28) A doctor will examine every patient.
(a doctor > every patient, every patient > a doctor)

The application of (27) to (28) based on its strict and literal interpretation should prohibit the application of QR for the purpose of obtaining the wide scope interpretation of the universal quantifier for (28) (contrary to fact), since the derivation in (28) can converge as it is (albeit with the existential quantifier interpreted with wide scope) under the natural assumption that QR does not involve any arbitrary uninterpretable features that may bear on some morphological reorganization. Turning to the assumption that the most elementary requirement of the CS/FLN is to make the interfaces possible and keeping to the interpretation side, Reinhart (2006: 102-103) goes on to take the interpretive paradigmatic situation from the viewpoint of economy to be one where "the more interpretations there are that can be associated with a given phonological representation, the more complex the computation at the context interface is." She (2006: 103) then arrives at the conclusion that at least from the perspective of the LF/context interface, "an economy strategy that would be extremely useful would be minimizing interpretative options associated with a given phonological representation," stating a rough approximation of the relevant principle as follows:

(29) *Minimize Interpretative Options*
Unless required for convergence, do not apply a procedure that increases the number of interpretations associated with a given single PF. (p.104)

According to the least effort principle in (27), QR is never allowed, being an illicit operation. But what Reinhart's (2006) interface strategies framework is exploring should be possibilities of constructing the system where illicit operations, such as QR, may still be used, in case the outputs of the CS/FLN are insufficient for the (LF-)interface needs. QR is here viewed as a marked option and hence a repair strategy to compensate for CS/FLN deficits for interface need purposes. Departing somewhat from Fox (1995, 2000), where the concept of optionality is regarded from the viewpoint of economy (which Reinhart 2006 adopts), Reinhart (2006: 108) observes that "... QR is not required for the interpretation of quantification, but it is only an optional operation for obtaining scope-shift. Whatever is needed for the interpretation of generalized quantifiers can be captured directly at the stage of assigning a semantic representation to sentences, as done in the Montague, or generalized-quantifier, tradition.... QR is an illicit operation that never applies at all, unless forced to by relevant interface needs." Then the interpretation of (28) with universal wide scope can only be obtained by applying the usually illicit operation of QR to it for the purpose of satisfying LF-/context-interface needs. Closing the discussion of "scope-shift" with QR as a repair strategy in the framework of Reinhart (2004, 2006), which I assume to be a good example of the weak version of the SMT, I would like to here again point out the same two set of problems that I located in the discussion of stress-shift: (i) the problem of how to treat QR, which is surely innate, but ranks lower than the usually "licit" operations?; and (ii) the problem of the very existence of reference-set computation in the human grammar, which I doubt.

While the empirical coverage of topics reported in Reinhart (2006), ranging over the phenomenon of scope-shift as an interface repair strategy, involving the problem of quantifier scope, the status of QR, etc., and the concept of reference-set computation as it applies to the PF problem of focus, anaphora resolution, the cost of processing, and the pragmatic notion of scalar implicatures, is surely remarkable and impressive, I would here like to make a hopefully plausible albeit necessarily incomplete attempt to present an alternative on the basis of Suzuki (2006) to the concept of reference-set computation. I intend to take up the long-standing problem of children's delay in the acquisition of principle B of the binding theory as a topic to discuss, comparing my treatment of the problem primarily based on Suzuki (2006) with Reinhart's (2006) in terms of reference-set computation. Before turning to the comparison itself, some observations relevant to the present concerns from the perspective of neurology may be in order. Although it is somehow couched in some version of connectionism, it may be interesting and instructive to see what Thompson and Varela (2001/2006) have to say in the following:

(30) a. Bi-directional and interactive relationship is predicted to exist between neural activities and the consciousness as a result of effects of general properties of emergence/creativity/intentionality in a system of complexity.
b. The crucially important process for the consciousness to emerge is not always restricted to events of neural activities within the bounds of the brain. It is a process crosscutting the domains of the brain-body-world. (p.83)

(31) a. The upward causal function to the effect that the whole is determined by the parts. New processes emerge with their own properties and the domains of temporal sequences and interactions.
b. The downward causal function to the effect that the parts are determined by the whole. The comprehensive properties of the entire system govern or constrain the behavior of the parts. (p.85)

I first make explicit some points relevant to the present discussion where some specific details pertaining to the framework of Reinhart (2004, 2006) have to be spelled out. Since the reflexivity framework of binding phenomena in the sense of Reinhart and Reuland (1993) and Reuland (2001) (among others) is adopted, principle B of the binding theory is assumed to comprise the A-chain condition to the effect that an A-chain must contain exactly one link which carries structural Case (at the head of the chain) and the reflexivity condition which requires a reflexive predicate to be reflexive-marked. Reinhart (2006) further assumes that intrasentential pronominal anaphora has two interpretations: binding and covaluation/coreference, taking "principle B" to be operative only in the former case, where the relevant pronominal is a bound one (with the logical syntactic version of the concept of binding: "α binds β iff α is an argument of a λ-predicate whose operator binds β"). Another important assumption of Reinhart (2006) should be "Rule I" in the sense of Grodzinsky and Reinhart (1993), which is an *intrasentential coreference* rule playing a role in constraining covaluation/coreference possibilities of pronominal anaphora within the bounds of a sentence. In a case where Rule I is involved, an instance of reference-set computation is observed in its interpretation, as in the following (Reinhart 2006: 211):

(32) Oscar hates him.

(33) *Reference set for covaluation/coreference*
a. Oscar hates him & him = Oscar.
b. Oscar (λ x (x hates x))

According to Reinhart (2006), a reference (comparison) set must be constructed because the third clause of Rule I ("The covaluation interpretation is indistinguishable from what

would be obtained if α binds β (a pronominal)") is not a simple structural condition on coindexation outputs, but an *optimality-type condition* which is supposed to compare more than one (possible) candidate sentences, including the one containing a pronominal in question, so as to compute the Rule I third clause. As for the example in (32, 33) above, the binding construal in (33b) is ruled out by principle B (see above for some discussion), while we have to construct the reference set in (33) in case we assign the free pronoun the value of *Oscar*, as in (33a), the bound interpretation in (33b) needing to be constructed and compared with (33a) in order for the Rule I third clause to be computed *even if the bound construal is ruled out by principle B.* Only if the two interpretations in (33a,b) are distinct in the relevant context is (33a) allowed, in accordance with the third clause of Rule I.

It may already be apparent what the central cause for a number of problems (to the extent they are considered to be problems) in the interface strategies framework of Reinhart (2004, 2006) is. These should surely arise from the very fact that Reinhart (2006: 1) has decided to allow computation of a strongly global, transderivational flavor, such as reference-set computation, to exist as a possible species of human language computation, *crucially for the purposes of computation at the interface.* My hunch is that there never is global, transderivational computation, such as reference-set computation, that could qualify as (part of) human language computation, at any *linguistic* level, including the relevant interface. My hunch/assumption is that everything linguistic from narrow syntax up to the interfaces must be of a local process, computational, largely modular, and innately specified (see Fodor 2000), perhaps with a varying degree of (un)consciousness depending on the domain to computation at hand in the fashion of Reuland (2001). I would take such reference-set computation with its global characterictics to be an ability of an obscure sort that might belong in the domain of the general learning strategy (GLS). As for the present concern of children's delay in the acquisition of principle B of the binding theory, Reinhart (2006) assumes primarily based on the processing account of Grodzinsky and Reinhart (1993) that poor performance on the part of children (with the proportion of correct responses on average about 50% of the time) with principle B largely derives from the assumption that the computation required by reference-set computation exceeds children's processing ability, specifically their working memory being limited for maturational reasons. (I return in my future work to Reinhart's 2006 problematic assumption that the 50% results on the part of children with principle B should derive from some kind of *guess performance by pure chance*, though the major part of the problem(s) arising from the 50% proportion may largely disappear in the analysis and exposition of Suzuki 2006, which is crucially based on the important experimental results of Kiguchi and Thornton 2004.)

I would refer the reader to Suzuki (2006: 47-59) for a possible alternative to Reinhart's (2006) processing account of children's delay of the acquisition of principle B on the basis of the limitations of their working memory, which would cause children to have a hard time dealing with reference-set computation. Since it has become clear thanks to Kiguchi

and Thornton's (2004) treatment of ACD constructions in the context of the acquisition of principle B of the binding theory that *principle B must be operative both in the case of the bound construal of the pronominal and in that of its coreference interpretation*, the account of children's 50% poor performance with principle B must be sought somewhere else. Perhaps the most intriguing portion with its important insight of Kiguchi and Thornton's (2004) experimental studies may be found in the fact that they explore principle B (im)possibilities in the extremely interesting empirical domain of ACD constructions, which crucially involve an elided portion that somehow has to be interpreted, with the quite remarkable consequence of obtaining a possible method of scrutinizing the way principle B may operate in a silent portion of a sentence. After examination of some major cases where ingredients of the phonology (e.g., stress assignment, which is observed to cancel effects of principle B) may affect semantic interpretation, I was led by these considerations in Suzuki (2006: 56) to propose the *default interpretation of the function of the phonology*, which is assumed to be operative in children at some relevant stages of language development, but not in adults (with possible implications for accounts of the evolution of language, as well), to the effect that "all properties of the phonology affect semantic interpretation." Crucially by combining this maturational principle with the general framework of Suzuki (2005, 2006) with the strengthened mapping hypothesis (SMH) as one of their central concepts, I hope to have in Suzuki (2006) shown children's delay in the acquisition of principle B to derive specifically from the presence of the maturational principle above and possible *efforts and time* needed to go back and forth between relevant components/modules so as to arrive at some requisite interpretation (see Suzuki 2005, 2006 for more details).

While we have seen above in (30, 31) some possible instances of neural activity/interaction in/across/out of the brain, I would here like only to touch on the implementation shown in (30a) in the hope of gaining some support for my account of children's delay of principle B of the binding theory briefly reported above and for the general framework of Suzuki (2005, 2006) with their SMH, more generally. Part of Thompson and Varela's (2001/2006) premise in (30a) runs as follows: "Bi-directional and interactive relationship is predicted to exist between neural activities and the consciousness ...." Although Thompson and Varela (2001/2006) appear to be couched in some version of connectionism, it may be not so implausible to conjecture that similar neural situations may exist in connection with language in the brain, with, of course, bi-directional/interactive passage constrained to a (far) larger extent in our theorizing than in theirs (2001/2006).

5 Some Preliminary Considerations on the Role of Dimensions in the Analysis of Human Language

Some well-known, much-discussed (in the literature) dimensional perspectives on linguistic

analysis can be found, mainly in the analysis of some empirical domains such as the coordinate structure construction (see Goodall 1987, Iwata 1998, for example), where a three-dimensional approach to the construction seems to be a favorite of the analyst who somehow sees all conjuncts of the coordinate structure to be stacked behind each other on the arguably two-dimensional plane, giving the impression that the configuration in which the whole coordinate structure arguably resides may constitute a three-dimensional space, and of the across-the-board movement phenomenon, where you find such examples as *Which book did John buy and read?*, with a single wh-phrase and more than one original position for it, the emerging image here again leading us to the impression that both conjunct VPs have to be three-dimensionally stacked with the rest of the structure represented on the two-dimensional plane (see Williams 1978 and relevant subsequent work). But my hunch in regard to the two constructions in question here is that both may be analyzed within the bounds of two-dimensional resources, but this may be possible only on condition that some "vertical" type of listing conjuncts of the coordinate structure is allowed as another legitimate way of representing (narrow) syntactic structures, in addition to the standard "horizontal" fashion of their representation. (See Uriagereka 1999 for the notions of "horizontal" and "vertical" ways of linguistic organization that correspond to "syntagmatic" and "paradigmatic" organization, respectively, and related discussion below, and also see below for the proposal for the role in linguistic analysis of *economy in the choice of a dimension*, according to which higher (than one) dimensional properties or resources can be allowed only if they are necessary.) We see below what Uriagereka and Pietroski (2002) have to say about possible roles the concept of a dimension may play in pursuing linguistic analysis specifically from the perspective of neo-Davidsonian event semantics, for the purposes of presenting some preliminary considerations on the role of dimensions in the analysis of human language and of identifying and establishing some plausible basic viewpoints in capitalizing on such a concept in an attempt to approach language from a rather non-traditional standpoint of the sort involved in Uriagereka and Pietroski (2002).

5.1 Uriagereka and Pietroski (2002)

According to Uriagereka and Pietroski (2002), the linguist's task is in large measure to uncover aspects of grammar not manifested in linguistic signals of the sort adopted (spoken, signed, perhaps "tactile" in the cases of Helen Keller and braille, or whatever), which are presumed to be primarily one-dimensional since their linear order is somehow time-bound. Some examples, where clear ambiguity is detected, are as follows: "She saw the man with binoculars" and "Flying planes can be dangerous." The following represents a case where more than mere differences in the linear order of constituents must be taken into consideration: "John thinks he likes spinach" vs. "He thinks John likes spinach."

These may require language to be at least two-dimensional, if only on condition that hierarchical relations count.

The major goal of Uriagereka and Pietroski (2002) is to explore two related theses: (i) The adjunct system is brutely concatenative and thus essentially flat (apart from asymmetries induced by the history of concatenation); but (ii) The thematic system brings in dimensionality and, as a consequence, nontrivial asymmetries.

Noting first the fact that language manifests all sorts of asymmetries, hierarchies, and subset conditions, and rejecting the description of these as "levels" taken to be either as primitives or dependent on some outside reality, Uriagereka and Pietroski (2002: 268) somehow come to suspect that what is called "the asymmetric nature of language" may reflect the different dimensionalities in its very fabric.[7] They (2002) also observe at the same time that their resulting arguments in their paper may shed some new light into the ongoing debate between atomists like Fodor (see Fodor 1970) and decompositionalists such as Pustejovsky (see Pustejovsky 1995). By way of explaining the more general question of why human language exhibits the asymmetries it does, Uriagereka and Pietroski (2002) consider a much-discussed fact in regard to causative constructions, namely, the "one-way" character of the typical entailments, as in the following:

(34) if *x* boiled *y*, then it follows that *x* did something that caused *y* to boil; but if *x* did something that caused *y* to boil, it doesn't follow that *x* boiled *y*. (p.268)

The central question here should be to explore possible reasons for the crucial fact that "the entailment is one-way." Then couching their discussion and analysis in the general framework of neo-Davidsonian "eventish" theory, they (2002) begin by discussing the following transitive/intransitive pair along with their neo-Davidsonian logical forms:

(35) a. Pat boiled the soup.
a'. $\exists e \exists x$ {Agent(e, Pat) & R(e, x) & Boiled(x) & Theme(x, the soup)}
b. The soup boiled.
b'. $\exists e$ {Boiled(e) & Theme(e, the soup)}

In (35a') "R" stands for some relation that an event *e* (done by the Agent) bears to the boiling of the soup. "Boiled" in (35a') captures the meaning of the intransitive verb in (35b) with the logical representation in (35b'). Uriagereka and Pietroski (2002) go on to point out that the question of why (35a) implies (35b) reduces to that of why the syntactic structure in (36) below has a meaning of the sort indicated by (35a).

(36) $[_{vP}$ Pat $[_{v'}$ *v*-boiled $[_{VP}$ (boiled) the soup]]]

They (2002) also point out that (35) is false in cases where (37a,b) can be true:

(37) a. Pat did something that caused the soup to boil.
b. Pat made the soup boil.

This raises the question of what the relation "R" in (35a') above stands for, not corresponding in its meaning to a simple (extensional and transitive) notion of causation. And the further questions to be addressed in any case should be: (i) Why isn't (35a) synonymous with (37a)?; and (ii) Why does (35a) have a natural meaning that restricts it to a sub-class of the cases that would verify (37a)?

Given (35a'), which is assumed above to be a possible semantic representation of (35a), the fact that the example below in (38) is not ambiguous in a way one might expect (either with the possibility of the adjunct modifying the "boiling of the soup" event denoted by x or with that of it modifying the "causing by Pat" event denoted by e; see Fodor 1970) leads one to the assumption that "R" between the causing and boiling events stands for a whole-to-part relation.

(38) Pat boiled the soup on Monday.

According to this assumption (with "R" as a whole-to-part relation), Pat in (38) is the agent of a complex "accordion-style" event whose final part is a boiling of the soup and whose first part is an action by Pat, and this event, which includes both Pat's action and the boiling of the soup, occurred on Monday (Uriagereka and Pietroski 2002: 270). The logical representations below in (39a,b,c) are taken to correspond to (35a, 35b, 38), respectively:

(39) a. $\exists$e {Agent(e, Pat) & $\exists$x[Terminator(e, x) & Boiled(x)] & Theme(e, the soup)}
b. $\exists$e {Boiled(e) & Theme(e, the soup)} (= (35b'))
c. $\exists$e {Agent(e, Pat) & $\exists$x[Terminator(e, x) & Boiled (x)] & Theme(e, the soup) & On Monday(e)}

In (39) "Terminator" expresses a kind of thematic role. If an event x is the Terminator of an event e, then x "participates in" e by virtue of being e's *final part*. In order for (39b) to strictly follow from (39a), Uriagereka and Pietroski (2002: 270) introduce a further assumption like the following, so that the Theme of an accordion-style event e is the Theme of any Terminator of e:

(40) Terminator(e, f) $\longrightarrow$ [Theme(e, x) $\longleftrightarrow$ Theme(f, x)]

Then Uriagereka and Pietroski (2002: 270-271) go on to turn to Fodor's (1970) question of why (38) does not have the interpretation below in (41), where Pat is the Agent of an accordion-event that ends with *a boiling of the soup on Monday?*

(41) ∃e {Agent(e, Pat) & ∃x[Terminator(e, x) & Boiled(x) & On Monday(x)] & Theme(x, the soup)}

Uriagereka and Pietroski (2002: 271) appears to attribute the impossibility of the interpretation in (41) for (38) to the impossibility as a structure of human language of the structure in (42) with *boiled* incorporated into the covert *v*, whose unacceptability remains to be seen:

(42) [$_{vP}$ Pat [$_{v'}$[$_{v'}$ *v*-boiled on Monday] [$_{VP}$ (boiled) the soup]]]

In connection with the question of exploring possible reasons for the unacceptability of (42), Uriagereka and Pietroski (2002) go on to discuss two approaches to sub-event modification. Noting the preliminary assumption concerning (sub-)event modification that "modifiers denoting open temporal events (like *for weeks*) can, if appropriately chosen, be used to say something about sub-events. By contrast, modifiers denoting concrete times (like *in 1995*) modify the whole event" (Uriagereka and Pietroski 2002: 271), they (2002: 272, 275) compare the following two hypotheses (along with the two assumptions in (44) pertaining to what they call "accordion" events):

(43) a. *Hypothesis A*
Some adjuncts can adjoin to verbs prior to incorporation and then go along for the ride when incorporation occurs.
b. *Hypothesis B*
All these adjuncts are formally predicates of the matrix event (corresponding to the post-incorporation transitive verb), but some predicates apply to an accordion-event by virtue of how certain parts of that event are related to the whole (as per Assumption One in (44a) below).

(44) a. *Assumption One*
If an event *x* is the Terminator of an event *e*, then *x* "participates in" *e* by virtue of being *e*'s final part.
b. *Assumption Two*
The Theme of an accordion-style event *e* is the Theme of any Terminator of *e*.

See above for some discussion on the two assumptions in (44). To clarify Hypothesis B (43b), Uriagereka and Pietroski (2002: 272) consider an old puzzle concerning sentences such as the following:

(45) a. Jack took the beans.
b. Jack took the beans intentionally.

Both (45a,b) are true if Jack tried to take the beans and did so. But if Jack successfully tried to take a box, which contained the beans without Jack's knowledge, (45a) is true but not (45b). It appears that the standard (neo-)Davidsonian event semantic logical representation like "$\exists$ e [Agent(e, Jack) & Took(e) & Theme(e, the beans) & Intentional(e)]" cannot accommodate further finer hierarchical details pertaining to various parts of an accordion-style event. To remedy the explanatory situation, Uriagereka and Pietroski (2002) go on to note that the first part of an accordion-event will typically be some action such as an attempt to do something by the relevant agent, with the following condition on accordion events:

(46) Agent(e, x) $\longrightarrow$ $\exists$ a [Initiator(a, e) & action-of(a, x)]

Under the assumption that actions are associated with Pietroski's propositional satisfaction conditions, an accordion-event e is intentional (as a first-pass approximation) if the condition associated with the action that initiates *e* is satisfied by the occurrence of *e* itself. In regard to the distinction between (45a,b), the condition may have the consequence to the effect that an event of Jack taking the beans is intentional if it starts with Jack trying to *take the beans*, while it is not intentional if it starts with Jack trying to *take a box*. Consider then the following (Uriagereka and Pietroski 2002: 271):

(47) Jack grew shiitake mushrooms for weeks at a time in the 1990s.

Notice that (47) contains two kinds of adjuncts, one modifying "internal, sub-events" (i.e., *for weeks at a time*) and the other modifying the larger matrix event (i.e., *in the 1990s*). It might be the case with (32?) that a complex event *e* shiitake-growing licenses the use of the adjunct *for weeks at a time* if *e* has sub-event parts of the same sort each of which lasted for weeks, showing Assumption One (44a) to be at work with the consequence of a single complex event composed of multiple episodes of shiitake-growing-by-Jack being both *in the 1990s* and *for weeks at a time*.

Regardless of whether you adopt Hypothesis A (43a) or Hypothesis B (43b), you have to come up with the reason(s) why certain adjuncts modify into sub-events, either through direct adjunction prior to incorporation-to-*v* (Hypothesis A) or by way of modifying the whole in a way that depends on certain part-whole relations, such as Assumption One (44a) (Hypothesis B). While adjuncts must be selective due to their lexico-semantic properties, Hypothesis A makes these selection properties relevant as adjuncts become active (namely, merged) pretty much at the point of the derivation that the sub-event they modify becomes merged (with the view decomposionalist), and Hypothesis B adjuncts manage to modify into the internal structure of complex predicates due to the part-whole make-up of these predicates a la Assumption One (44a), a view more congenial to the atomist approach (see Fodor 1970).

Considering the effect of (Spanish) clitic climbing on sub-events, Uriagereka and Pietroski (2002: 273-274) discuss and compare the decomposionalist and atomist approaches to lexical items in the empirical domain of contextual anchoring of sub-events on the part of the clitic, on the basis of the analysis of the following Spanish sentences:

(48) a. El terrorista destrozó el corazón del paciente.
the terrorist destroyed the heart of-the patient
b. El terrorista le destrozó el corazón al paciente.
the terrorist DAT destroyed the heart to-the patient

Their (2002) story concerning the sentences in (48) is like this: "Somewhere, at high noon, a terrorist launches a missile that strikes a hospital at 12:20. In that time interval, doctors remove a patient's heart (which was in its original place at 12:00) and store it in a laboratory at the other end of the hospital, while a new heart is placed (at 12:15) in the patient's chest. The missile strikes and the following Spanish sentences are uttered in the news to describe the horrifying event" (p.273). While both sentences in (48) mean roughly "the terrorist destroyed the patient's heart," it is not so easy to translate the sentences into English, specifically with minute interpretive details for the sentence in (48b). As it turns out, Spanish has a way of determining whether "the patient's heart" in question is the one in the chest. The sentence in (48a) does not show this kind of subtlety, with pretty much the import of the English counterpart. Important is the sentence in (48b), which definitely means that "the destroyed heart is the new heart, not the old one." (See Uriagereka 2001 "Doubling and possession" in *Clitics in phonology, morphology and syntax*, ed. by Gerlach and Grijzenhout for the precise mechanisms for why (48b) has the interpretation it does.) Descriptively, the clitic *le*, which refers to the patient, serves as *contextual anchor for the heart's destruction*, namely, as indicator for determining that *the locus of the heart's destruction resides in the patient, i.e., in the patient's chest*, as some ingredients of Spanish grammar ensures (see above). And since the sentence in (48b) implies that "the destroyed heart is the heart at the patient, but not just any old heart," we obtain the intended semantics associated with the presence of the clitic in such sentences as (48b).

Uriagereka and Pietroski (2002: 273-274) stress at this point that the verb *destroy* has the rough logical form of *boil* above, lexical differences aside, which means that the sentences in (48a,b) constitute accordion-events of the type discussed above, with the concomitant Spanish semantics associated with the interpretation of the clitic *le* somehow ensuring that the clitic contextually anchors just the destruction part. According to their (2002) missile story above, the heart which would be hit was not attached to the patient's body yet at the time of the missile's launching by the terrorist (at 12:00). Then it should somehow be the case that the clitic *le*, which refers to the patient, serves as contextual anchor for the destruction part (as the final part of the accordion-event roughly consisting of the causing and destroying sub-events in (48b)). The question is how contextual

anchoring of the embedded sub-event takes place.

Hypothesis A in (43a) would have it that since it is possible for an adjunct to adjoin to a verb prior to its incorporation into the covert transitive *v*, the context-confinement via the clitic of the final, destroying sub-event can take place without affecting its associated, causing sub-event (the decomposionalist view). Let us then see how Hypothesis B (43b) can also be instrumental in accounting for this situation involving the clitic *le* (the atomist view). The key observation from this perspective is that we cannot just contextualize the destruction part, ignoring the rest of the accordion-event (modification being inaccessible to any sub-event during the derivation, a la atomism). What is crucial seems to be the heart in the patient's chest when the event *ended*, which heart was in the patient's chest when the accordion-event *started* being completely irrelevant to the aim of the clitic *le* serving as contextual anchor for the heart's destruction. The lexico-conceptual contribution of the complement *the heart* is tied to the intransitive (pre-incorporation) verb *destroy* . So the relevant heart is the one *in the patient's chest when its destruction takes place*, and, according to Assumption Two (44b), *the heart* is the only theme for the accordion-event at large. Once again, this atomist perspective allows no direct contextualization of the internal sub-event.

Turning to the internal make-up of events, Uriagereka and Pietroski (2002: 274-276) initially observe that for the purpose of modification or contextual anchoring of them, internal sub-events must somehow be represented either at some initial/intermediate syntactic level (for Hypothesis A) or at logical form (for Hypothesis B). They (2002) then explore the nature of that representation, suggesting that it has *dimensional* characteristics. Intuitively, as an n-dimensional apparatus underlies an n+m one, so the presence of *a causative layer* in an accordion-event should signal that of *lower layers*. While Hypothesis A does not seem to have much trouble with this sort of reasoning, the different syntactic layers corresponding to the various dimensions, it may be the case with the atomist view that it is the specific information make-up congenial to it that even Hypothesis B allows us to recover the presence of information in the interior of the larger event, the make-up in question presumably not being syntactic (in the sense of being available for various syntactic operations), but having enough resources to support modification and contextual anchors. The relevant expressions are presumed here to exhibit dimensionality that is not initially obvious, and according to Uriagereka and Pietroski (2002: note 9), lexico-conceptual structures occupy a separate syntactic component of the system (the decompositionalist view), whereas they are a mere aspect of some other level (the atomist view).

By way of confirming the validity of the information make-up alluded to above in connection with Hypothesis B, Uriagereka and Pietroski (2002: 274-276) go on to observe that the main argument for its validity derives from the fact that it can provide an explanation to Assumption One in (44a) above to the effect that "If an event x is the Terminator of an event *e*, the *x* 'participates in' *e* by virtue of being *e*'s final part." (See also

Assumption Two in (44b) to the effect that "The Theme of an accordion-style event *e* is the Theme of any Terminator of *e*.") Then consider the following paradigm:

(49) a. The door was open.
b. The door opened.
c. Jack opened the door.

The inferential relations among the examples in (49a,b,c) may suggest a hierarchical relation among the adjectival, intransitive, and transitive forms of *open*. Note the preliminary intuition that "events (as opposed to states) introduce the idea of change over time, and processes somehow 'extend' that idea to introduce Agents responsible for the change" (Uriagereka and Pietroski 2002: 275). The adjective *open* (see (49a)) may be conceived of either as a predicate of individuals (with the meaning: "the(x): door(x) [Open(x)]") or as a predicate of states, which are thought of as eventualities that hold through time (with the meaning: "∃s [Open(s) & the(x): door(x) [Theme(s, x)]]). Both approaches allow us to think of events as changes (of state) in individuals, treating intransitives as predicates of changes (see (49b)) and transitives/accordion-events as processes that terminate in an event of some individual coming to be in the relevant state (see (49c)). Somewhat sophisticated and informative representations for the predicates/predicatios of the paradigm in (49) of the neo-Davidsonian event semantics sort may be like the following:

(50) a. ∃s <Open(s)> STATE (adjectival)
b. ∃f [∃s <Change(f, s) & Open(s)>] EVENT (intransitive)
c. ∃e {Agent(e, x) & ∃f [Terminator(e, f) & ∃s <Change(f, s) & Open(s)>]}
PROCESS (transitive)

Adding a "Theme"-conjunct to each of the semantic representations in (50) may complete the entire propositions. Clearly, (50c) implies (50b), which implies (50a). While there appears to be no particularly strong reason for taking the representations in (50) to be dimensional in nature, as Uriagereka and Pietroski (2002: 276) note, the crucial assumption here should be that in (44b) above, the central intuition behind which is the idea that an eventuality constituting the content of a whole accordion-event is somehow built around a "lower" Theme (i.e., the Theme of the "pre-incorporation" intransive verb *open* in (49c), for example). Look at the following thesis:

(51) Terminator(e, f) ⟶ [Theme(e, x) ⟷ Theme(f, x)] (= (40))

The thesis in (51) states that if we extend a simple event *f* into a process *e*, then the Theme of the whole process (i.e., the accordion-event) is just the Theme of the simple event. According to Uriagereka and Pietroski (2002: 276), the thesis does not follow on any

simple-minded interpretation of the hierarchy in (50) and accounting for Assumption Two (44b) may evidently require a hierarchy that arises as a result of its levels in some sense being defined in terms of dimensions involving the Theme space. (Assumption One (44a) may well be conceived of in a similar fashion.)

Noting that the apparatus implicit in (50) may, in fundamental respects, be like the one underlying familiar notions from geometry or arithmetic, Uriagereka and Pietroski (2002: 277-78) go on to ask whether specific lexical items (or something else in the grammar) are responsible for these dimensional shifts. Look again at (52) and its neo-Davidsonian semantic representation in (53):

(52) Pat boiled the soup on Monday. (= (38))

(53) $\exists$ e {Agent(e, Pat) & $\exists$ x [Terminator(e, x) & Boiled(x)] & Theme(e, the soup) & On Monday(e)} (= (39c))

Boiled (the derived transitive verb), the adjunct *on Monday*, "Agent(e, Pat)," "Theme(e, the soup)" are monadic predicates of events, while *Pat, the soup*, and *Monday* make their semantic contribution as arguments to binary predicates ("Agent," "Theme," and *on*, respectively) that express a relation between events and other things. Note also that arguments, adjuncts, and verbs are all treated alike, that is, semantically as conjuncts of a complex event description; i.e., phrase markers are interpreted as conjunctive predicates in a framework of neo-Davidsonian event semantics.

Observing that since arguments such as *Pat* and *the soup* are not predicates of events by themselves, they have to be interpreted via thematic roles like Agent, Theme, and so on, and that in the sentence "Pat boiled the soup on Monday," neo-Davidsonians require the subject *Pat* and the object *the soup* to be interpreted through a limited kind of type-shifting as the monadic event predicates "Agent(e, Pat)" and "Theme(e, the soup)," respectively, Uriagereka and Pietroski (2002: 277) note that this kind of situation is not surprising in so far as "event analyses are designed to account for the compellingness of inferences involving adjuncts, "Pat boiled the water on Monday, so Pat boiled the water," as instances of conjunction-reduction." Then Uriagereka and Pietroski (2002: 278) emphasize the significance of no language we know of having any lexical items synonymous with the metalanguage expressions "Theme," "Agent," "Benefactive," and so on. While languages have words for tense, force indicators, a variety of quantification expressions, and many others, they do not lexically represent what seems to be a central part of their vocabulary (i.e., thematic roles). In connection with the correlation of Case-markers with θ-roles, look at the following:

(54) a. I like *him*.
b. I lied to *him*.

c. I believe *him* to be a genius.
d. I literally used *him* as a counterweight to lift the piano.
e. I lifted the piano with *him* as a counterweight.

(55) I like *Theme*-him, but I used *Instrumental*-him to lift the piano.

Apart from the case of a handful of restricted, so-called lexical Cases, the accusative *him*, for example, can bear all sorts of θ-roles (see (54a-e)). The remarkable situation, however, is that there seems to be no language that distinguishes *him* in, say, (54a, d), as in (55).

According to Uriagereka and Pietroski (2002: 278), the fact of this sort seems to reveal a simple truth: θ-roles are not part of the language under analysis; that is, human languages do not have everyday lexical items with the meanings of "Theme," "Agent," and so on. Notice that neo-Davidsonian event semantics has it that the normal operation for semantic representational composition of human language consists only of "utterly trivial, dull predication," with the entire event itself and perhaps other things as arguments of largely monadic and sometimes binary (specifically, in the cases involving θ-roles) predicates, which constitute the series of coordinate conjuncts with "&" as the conjunction in the domain of existential closure associated with the entire event.[8] And Uriagereka and Pietroski (2002) go on to observe that the presence of θ-roles enables human language to "step out" of the simple-minded predicative routine, making it possible for it to "describe domains (and causal relations) with elaborate structures" by capitalizing on "otherwise simple linguistic expressions, initially 'designed' for simple predication." More technically, a θ-role can be taken to be a type-lifter, which raises the type of an argument (e.g., *Pat*, *the soup* in (38/52)) to that of a predicate. That is, the arguments *Pat* and *the soup* in (38/52), for example, are raised to the status of predicates by entering the binary predication with "Agent" and "Theme", respectively, as predicates, with "Agent(e, Pat)" and "Theme (e, the soup)," respectively, as resulting monadic predicates of events (see (39c/53)). Since Agent(e, Pat) and Theme(e, the soup) are predicates, they can relate appropriately to other predicates in an event semantic representation. According to Uriagereka and Pietroski (2002), whenever the language system uses a θ-role (namely, a type-lifter, with its argument e and value *Pat* in the case of "Theme" in (38/52), for example), it must be the case that it is stepping out of its boundaries, presumably out of its "syntactic" boundaries, due to the fact that θ-roles are not syntactic formatives and also that they are external to the lexicon. They (2002) go on to conclude at this point that "it is natural to think of these external-to-the-lexicon items as co-extensive with dimensional cuts."

While Uriagereka and Pietroski (2002) themselves admit that their arguments for dimensions so far are only plausibility ones, their tentative picture here of θ-roles in connection with the concept of a dimension is roughly that dimensions are points where θ-roles come in, the resulting representations being tightly articulated in interesting ways with part-whole implications built around the notion of a Theme space. Continuing

to pursue a dimensional perspective on linguistic analysis, they (2002: 278-280) turn to the exploration of the "place for adjuncts." Based on the earlier work of one of the authors, which is an attempt to develop an argument suggested by Chomsky in recent class lectures (spring 2001), they (2002) first assume that "adjuncts inhabit their own (especially simple) dimension," pointing out one of their remarkable characteristics to the effect that *they do not have labels* and inquiring how the system tells apart a syntactic object with adjunct X from the same syntactic object with adjunct Y, given the assumption above that adjuncts do not have labels. While it may be possible from a neo-Davidsonian semantic perspective to assume that an adjunct is "just there" as a mere conjunct in an event representation, the problem remains of what formal properties (if any) the grammar tracks in dealing with the relevant object, a syntactic object with adjunct X or Y, under the assumption that (by hypothesis) it does not track sub-parts of the syntactic object by their labels. On the basis of the examples with a number of adjuncts added to them (i.e., "Beans grew for weeks, for years, for decades," "Jack [grew beans for weeks, for years, for decades ...] twice, three times, four times ..."), Uriagereka and Pietroski (2002) suggest that adjunctions stem from a very simple, plainly concatenative method of expanding linguistic expressions, unlike the richer and more constrained hierarchical thematic system. Essentially, adjuncts are just derivationally added to the current phrase-marker without ever being structurally attached to it, no issue then arising about their bare phrasal status (i.e., either specifiers or adjuncts) and lack of their transformational syntax (with wh-movement of them destroyed by the weakest of islands, for example) following in a fairly transparent fashion. Moreover, the semantic scope of each adjunct (e.g., "Beans grew for weeks, for years, for decades," with the interpretation, specifically for the adjuncts, on which "beans grew for weeks, and they grew for weeks at a time for years, and they grew like this for decades") for compositionality purposes derives from the sheer order of its activation in the derivational workspace.

Uriagereka and Pietroski (2002: 280-283) then turn to the very recalcitrant, albeit intriguing descriptive concept of "infinite regress," which pertains in large measure to the much-discussed antecedent-contained deletion (ACD) construction and led May (1985) to take advantage of the operation of quantifier raising that would give the effect of (covertly) moving the elided (VP) portion out of its antecedent VP (with the choice of its landing site subject to economy), so as to ensure interpretation of the sentence containing an ACD construction by thus avoiding "infinite regress." Before starting the discussion of "infinite regress" itself, they (2002) first note that the scope of modification may be another empirical domain where adjuncts are very different from arguments. In case two (or more) adjuncts are simultaneously activated/merged in a derivational workspace, they ought to show no relative scope differences, sometimes going by the name of "disjuncts." Then consider the following:

(56) a. Lawyers behave nicely rudely.

b. Lawyers behave rudely nicely.

According to Uriagereka and Pietroski (2002), the two sentences in (56) mean the same thing (something paraphrasable as: *lawyers behave nicely, rudely ..., rudely, nicely ... who knows? both ways, as life is messy in court*) on a certain, open-ended reading, which may be quite different from the one on which obvious differences in meaning between the two, presumably due to scope differences of some sort, can be detected. They (2002) then somehow conclude from this and observations so far that "different sorts of adjuncts associate with different dimensionalities" and that this kind of situation may have to do with the "possibility that sentences containing adjuncts could be, in some non-trivial sense, *infinitely long* (emphasis — *NS*)."

The above observations on adjuncts, specifically on what are called "disjuncts" here, may point to the (presumably) factual situation where the class of sentences containing them, which would be transfinite and thus of a different dimensionality from that of sentences not involving them. For it would not make any sense in Uriagereka and Pietroski's (2002) terms to have sentences with infinitely many arguments, under the assumption that these require transformational syntax to converge (i.e., in terms of Agree, etc.; perhaps, with the finiteness of the number of its arguments with respect to a given head also explainable in terms of general lexical properties of human words (specifically, those which can serve as heads)). A transformation maps a definite input to a definite output, thus being incapable of involving infinitely long inputs or outputs. However, since disjuncts, adjuncts more generally, by hypothesis do not require transformational syntax to converge, there should remain no problem with the size of sentences that contain them.

Noting the obvious impossibility of constructing a *physical* sentence with phonetics and linear order involved, Uriagereka and Pietroski (2002) turn to pure LF representations without immediate concern over associated PF ones. They (2002: 281) then take up the so-called "infinite regress" puzzle arising in ellipsis phenomena with special emphasis on the antecedent-contained deletion (ACD) construction, given the quite intriguing question of what sort of intuition speakers have about the "infinite regress" factor both when the relevant construction involves disjuncts and when it involves arguments. The standard device for avoiding an infinite regress is quantifier raising (QR) in the sense of May (1985) (among others), so that the (quantified) phrase containing the elided material can scope out of the antecedent VP containing it.[9] Since resort to QR should necessarily erase possible distinctions that they (2002) want for some exposition of the two different cases (i.e., ACD with disjuncts and ACD with arguments), Uriagereka and Pietroski (2002) turn to cases where QR cannot apply (after considering some "intermediate" cases; see Uriagereka and Pietroski 2002: 281-282). Consider the following somewhat forced examples in this connection:

(57) a. Inevitably Monday follows (on) Monday, which Sunday does as well, etc.
b. Inevitably Monday follows the day before Monday, which Tuesday does too (etc.).

Note that (57a), where the head of the non-restrictive relative construction is an adjunct, is relevant to ACD with disjuncts and (57b), where it is an argument, is relevant to ACD with arguments. As for (57a), etc. has been added to suggest an open-ended, rising intonation on *as well*, thus allowing the disjunct interpretation. According the speaker Uriagereka and Pietroski consulted, (57a) can roughly mean: "Monday (intransitively) follows on Monday, which Sunday (transitively) follows as well on Monday, which Sunday (transitively) follows as well on Monday, etc." (Notice the interesting mismatch in (in)transitivity of the verb involved between the elided portion and the antecedent.) This interpretation of (57a) somehow invokes the infinity of time, establishing that *sentences involving an infinite regress with adjuncts are acceptable and interpretable.* Uriagereka and Pietroski (2002: 282) then go on to discuss the case of a sentence involving an infinite regress with arguments. The example in (57b), where *which* crucially modifies *Monday* and not *the day before Monday*, is an ACD construction with arguments where neither QR nor any other movement can apply, thus lacking any viable means of saving itself. Moreover, even if (overt) A-movement of some sort carries something out of the VP in (57b) and thus resolves the infinite regress problem with (57b) (note further that there are cases where (some species of) QR may raise non-quantified NPs in sentences such as "The Mermaid baked him [the same food that Cookie Monster did]," where the bracketed portion QR-raises out of the antecedent VP; see Kiguchi and Thornton 2004: 253), that ought to be *the day before Monday*, presumably *Monday* being in an adjunct phrase, and it should be clear that this movement could not provide a relevant ellipsis. While the putative *post-QR* (pertaining to *Monday*) semantic representation for (57b) where the infinite regress problem had been solved via (factually impossible) QR of *Monday* would be something like "Inevitably Monday follows the day before Monday, the day before which Tuesday follows too" (which itself is unacceptable, being unable to avoid the infinite regress problem because of the unavailability of requisite movement due to an adjunct condition of some sort and also unable to support infinite regress thanks to the fact that the head of the non-restrictive construction in (57b) is an argument), the representation we obtain in the case of A-movement or QR of *the day before Monday* would be "Inevitably Monday follows the day before Monday, which Tuesday follows too" (which has escaped the infinite regress problem via some movement of the sort above and itself is acceptable, though with a meaning different from that intended here for (57b)), which cannot express the intended interpretation of (57b), on which "Tuesday follows (the day before) Monday," but not "Tuesday follows the day before Monday" (with the discussion necessarily quite intricate and subtle, given the natural difficulty of constructing ACD examples, both grammatical and ungrammatical, that are not subject to QR for their interpretation for the purposes of examining the (un)availability of infinite regress; see Uriagereka and Pietroski 2002:

282-283 for more examples and discussion). And the most fundamental question here is again why the sentence in (57b) cannot just mean: "Inevitably Monday follows the day before Monday, the day before which Tuesday follows too, the day before which Tuesday follows too, the day before which Tuesday follows too, etc.," where *which* crucially modifies *Monday* (with the movement of *Monday* to escape infinite regress ruled out by an island condition of some sort). This impossibility of the infinite regress interpretation should lead us to conclude that when the head of the non-restrictive construction is an argument, infinite regress is not allowed; namely, arguments disallow infinite regress, more generally. As we have seen, infinite regresses are impossible with arguments, while they are possible with adjuncts, more specifically with disjuncts. Uriagereka and Petroski (2002: 283) stress in particular that "a system that allows infinitely long expressions ought to be of a higher dimension,"

Recapitulating by way of pointing out problems to be further pursued, adjunction has been taken to be a more basic syntactic relation than Merge, and correspondingly, predication should be a more elementary semantic notion than θ-role assignment (in the general frameworks of minimalist syntax and neo-Davidsonian semantics). The general assumption here has been that *adjunction is flat*, whereas *hierarchies emerge as a result of argument taking*, thus creating new representational spaces or dimensions, and it should be through dimensional shifts associated with argument taking that *asymmetry enters the picture*. "That suggests a kind of open-endedness for a chunk of language that can only be understood biologically if the system is so underspecified that it has virtually no cognitive limits, and thus is, in itself, relatively limited as a system of thought, communication, and so on. Yet that very system, with a minor improvement (argument taking) all of a sudden becomes constrained, plastic, creative in useful terms, and otherwise familiar as *our* human language" (Uriagereka and Pietroski 2002: 284).

Notes

* This paper, which is surely my longest one thus far, is dedicated to the memory of Masayoshi Iseki, our dear long*est*timer, a friend and colleague of mine and, of course, of many others' as well at Kobe Shinwa Women's University for many irreplaceable, brilliant years, who died a sudden, premature, untimely death from a heart attack on the early morning of December 15, 2006, after a usually long day and night of hard and sincere work dedicated to University. The rest of us, who are left behind, have to face and cope with a great number of extremely difficult problems arising from the hard reality surrounding us, so as to survive as University, an awful, near insurmountable feat, *without him*. May his soul and his family left behind be soothed, and may we/University, who are also left with the hardship of life ahead, *without him, but somehow with him within us*, be guided along the right lines in the coming troubled times to be encountered.

[12/16/06 writ]

The portion of the present article where the extremely intriguing case of Helen Keller is discussed, primarily in order to rebut Gill's (1997) (largely misguided) criticism on the basis of some

version of "body theory" of the general framework of Chomsky's generative grammar as it applies to the explanation of the child's (first) language acquisition and, specifically here, to the exposition of the case of Helen Keller, grew out of my 2006 spring semester class lectures. Thanks to my students who attended my lectures. But more importantly, the present paper can be taken to be my first attempt to explore the ways different dimensions could be capitalized on to express a variety of things, results obtained here being necessarily poor and incomplete, given the current understanding of the concept and function of dimensions in human language. I have greatly benefited from frequent discussion with Takaharu Hirai (the "Mathematical Magician") on the recalcitrant problem of "What exactly is a 'dimension' as it pertains to natural language?" Special thanks to him, Hideo Ohashi, from whom I was able to obtain Chomsky's most recent manuscript on UG and the SMT, Seishi Matsuda, who led me through constant discussion almost always at my convenience to the better and deeper understanding of the general framework of "anthropology of symmetry" (deriving from religious anthropology) of Nakazawa (2002, 2004), another important albeit poorly discussed ingredient of the present paper, which has the potential to bridge Neanderthals and *Homo sapiens* based on recent results from cognitive archaeology, equating the Unconscious in the sense of Freud with language, both of which are arguably present in *Homo sapiens*, but not in Neanderthals, and, crucially, also identifying the (mis)use of these new human devices as the source of (excessive) "asymmetry," which concept should without doubt constitute perhaps the strongest defining property of the present world, where there seem to be few viable resources to get rid of the poverty and violence prevalent everywhere that arise from the plethora of excessive greed on the part of the ruling few, and Toshihiko Kobayashi, constant interesting discussion with whom on intriguing theoretical questions I admit should count as one of the greatest pleasures of mine in my academic life. The rest of the paper is largely couched in the general minimalist framework of Suzuki (2005, 2006).

1 Epstein and Seely (1999: 4) cite J. Epstein's observation: "direct theoretical appeal to macrostructure properties fails to explain macrostructure."

2 Kayne (1994: 5-6) takes asymmetric c-command to be the concept that is closely matched to the linear ordering of the set of terminals and employs the notion of the nonterminal-to-terminal dominance relation (dubbed *d*), which is a many-to-many mapping from nonterminals to terminals, calling d(X) the set of terminals that a nonterminal X dominates. d(X) can be said to be the "image" under *d* of X. Then considering the set *A* of ordered pairs $<X_j, Y_j>$ such that for each j, X_j asymmetrically c-commands Y_j and further taking *A* to be the maximal such set (with *A* containing all pairs of nonterminals such that the first asymmetrically c-commands the second), Kayne goes on to propose the following:

(i) *LCA* (*Linear Correspondence Axiom;* Kayne 1994: 6):

d(*A*) is a linear ordering of *T*. (with *T* the set of terminals)

3 C-I: "conceptual-intentional"; H: "head"; OCC: "occurrence," corresponding to the notion of EPP-feature in standard terminology and, possibly, to the concept of edge-feature (EF) in the sense of Chomsky (2005b).

4 While it may be basically correct in evaluating different versions of the FLN-interface connection/SMT to take Chomsky's (2000a, etc.) version to be standard and Frampton and Gutmann's (2002, etc.) crash-proof system a stronger version. But if crashed derivations are interpreted as gibberish (see Chomsky 2005a), then every derivation may somehow be interpreted in Chomsky's system as well, the relevant notion not being convergence, but the *availability of an interpretation.* Then Chomsky's (2000a, etc.) may also be a stronger version of the SMT.

5 It may not be implausible to assume that *economy* considerations of some sort are involved in the *choice of a dimension* in distinct aspects of language, since "language" is also a phenomenon in the

world and economy can be considered to be one of the leading and guiding tools in the evaluation of a great many different aspects of the world. In principle, apart from the phonetics/phonology that has to be four-dimensional (due to properties of "time" as it exists in this world), many aspects of the other components pertaining to language (syntax, semantics, pragmatics, etc.) can be any-number-dimensional, with the economy hierarchy ranging from costly higher dimensions to the cheapest first dimension. Higher (than one) dimensional properties may be allowed only if they are (empirically) needed. (Space-wise, you can see lower-dimensional entities (i.e., one-, two-, (and three-) dimensional), but not higher-dimensional ones (e.g., four-, five-, etc. -dimensional).)

6 Some speculation on what we call "brain death" may be in order here (see also section 5 in the text). I would assume that the *Homo sapiens* brain can go beyond the third dimension due to the presence of the Unconscious/FLN, reaching at least the fourth dimension. Under the assumption that while *brain-wise* you need resources belonging in the fourth or higher dimensions if you hope to properly and sufficiently function in the *Homo sapiens* manner, you should remain *body-wise* within the bounds of up to the third dimension (because the body is encaged in this three-dimensional world), the factual situation pertaining to the so-called "brain death" may amount to one where only its resources coming from dimensions higher than the third cease to properly function when the brain dies, whereas the body arguably residing in the third dimension can continue to live even after the death of the brain, as a result of which only resources belonging in dimensions higher than the third have been lost (in some sense yet to be clarified). Note that given this speculation on what is called "brain death," we may be faced with at least two quite intriguing problems like the following: (1) Why can the brain alone have any potential to go beyond the third dimension, given our assumption when we discarded traditional dualism with the dichotomy between the body and the mind/-brain that both of them should be considered and analyzed in terms of the single concept of "physicality," whose (original) idea must be grounded solely on resources, factors, and elements coming from our three-dimensional world; and (2) Does admitting that the body continues to be alive at the time the brain is dead (in some sense) not reintroduce traditional dualism that we had hard time getting rid of? As for question (1), I simply note here that the *Homo sapiens* brain endowed with the Unconscious/FLN somehow came to possess "energy" of some sort, yet to be explained, but with some potential to break into a higher dimension (partly due to lack of my deep understanding of recent results of many relevant empirical sciences, particularly of physics, and also partly because of my present reluctance to go into any mystical or religious knowledge). Question (2) does not seem to bring about much confusion or difficulty. My interpretation of "brain death" simply says that only resources belonging in dimensions higher than the third "die." It does not say anything about whether or not the body or the brain is dead or alive. It only mentions the difference in dimension. Since at the time of "brain death" only resources belonging in dimensions higher than the third "die" and it is only the brain that has such resources, which serve to enable the brain to operate in the *Homo sapiens* manner, the resulting surface picture should be such that while the brain is dead, the body is still alive, the actual fact being that the dead portions of the brain are only those belonging to higher dimensions (and the non-*Homo sapiens* portions belonging to the third dimension being *in fact alive*) and, of course, that the body, which only belongs to the three-dimensional world, is definitely still alive. Then much talked-about cases where a dead man, specifically one who has died a "brain death," has returned to life should be no wonder, given the assumption above that those portions of your brain (and your body) belonging to the third dimension are arguably still alive even after you have been judged to be dead (due to "brain death"). Only, those people who have returned from "death" should be in an "animal-like" state due to the absence of resources belonging in dimensions higher than the third that used to maintain and support the *Homo sapiens* traits (including language, among other things). But, of course, this speculation from the perspective of dimensional difference on the possible human "after-death" state as a result of "brain death" must be subjected to rigorous scrutiny on the part of many relevant empirical

sciences. (As is usual with every discussion so far on the brain system in this paper, its nonlocal portions/aspects in the sense of Fodor 2000 are excluded from the discussion here as well.)

7 See, for example, the following semantic subset principle that establishes children's initial semantic hypotheses, which can be falsified on the basis of positive evidence from the input, under the assumption that children lack negative syntactic evidence:

(i) *Semantic Subset Principle*

Suppose that the interpretive component of Universal Grammar makes two interpretations, A and B, available for a sentence, S. If so, then see if S is true in a narrower range of circumstances on interpretation A than on interpretation B. If so, then A will be hypothesized before B in the course of language development. (Crain and Thornton 1998: 118)

8 While the canonical form for semantic representation in the framework of neo-Davidsonian semantics is begun with an existential operator associated with the entire event and it is interesting for Uriagereka and Pietroski (2002: 269-271) to compare such neo-Davidsonian semantic representations with syntactic clausal structures, specifically up to vP, v (a functional category; abstracting away from a possible distinction between v and v*) eventually playing a major, albeit indirect (since it is a syntactic notion) role in establishing the notion of "accordion-event," it may be instructive to recapitulate major functions of (lexical and) functional categories from a syntactic perspective. I adopt on the basis of the past literature (and my own evidence relevant to the discussion here, which will be explored in future works) the stronger assumption that *all* functional elements are discourse-linkers in the sense that their contents must be licensed by some resources from the discourse or the context (presumably constituting the A'-domain in the A/A'-dichotomy). Moreover, I take the edges in the sentential structure (i.e., the C-edge, v-edge, and, perhaps, D-edge or n*-edge as well; see Chomsky 2006 for some discussion pertaining to a nominal edge) to be the loci through which functional elements can link with the discourse or context, which situation should require them to move to such edges either overtly or covertly (perhaps, with the latter distinction constituting possible crosslinguistic parameters; see the distinction between overt vs. covert *wh*-movement, for example). Some exemplary functional discourse-linkers are: (i) DPs (but not NPs), which must link to the context even when they appear alone in the discourse (e.g., the DP "That student," uttered in some context, must be checked against some set of candidates for the target student appearing in that context, with the student set containing at least one student and *that* raising to n* for the purposes of linking to the context; even proper names such as "Mary" and "John" are taken to undergo N-to-D raising covertly in the case of English in the framework of Longobardi 1994, presumably for the major purposes of functioning as constituents in the sentence in which they appear, and also for the purposes of being licensed as discourse entities, perhaps with the necessary licensing/identification implemented in terms of something like Heim's (1983/2002) file change semantics); (ii) "agreement (e.g., person, number)," specifically with the first and second persons necessarily interpreted against the scene on which the relevant conversation is under way (note the sharp contrast between agreement and Case, the latter of which may presumably never be a discourse entity, not even morphological Cases); and (iii) focus, topic, specificity (including interesting problems with the transparency/opacity question pertaining to the interpretation of indefinites and, presumably, also to the proper understanding of the nature of the relations holding between (both deictic and non-deictic) Tenses in so-called "sequence of tenses" constructions; see also Reinhart 2006: 91-101, among others, for some interesting motivation to take indefinites allowing free wide scope to be subject to a choice-function interpretation), pronominal anaphora (see Suzuki 2006: 47-59 for extensive discussion in the general framework of Suzuki 2005 crucially involving the "strengthened mapping hypothesis (SMH)" on "children's delay in the acquisition of principle B of the binding theory" that tries to account for Kiguchi and Thornton's 2004 findings indicative of the

full operation in ACD constructions of principle B both in the case where the antecedent is a referential NP and in the case where it is a quantified NP), and many others.

9 It may be instructive to see at this point what Chomsky (2004: 120-122) has to say in connection with adjunction, ACD constructions, infinite regress, QR, and so on, particularly because Uriagereka, one of the authors of the paper under discussion, was greatly inspired in this connection by Chomsky's 2001 spring semester class lectures. Taking up the ACD construction, in particular (with subsequent, interesting results for the other concepts: adjunction, infinite regress, QR, and so on), Chomsky (2004) points out some serious problems with standard ACD resolution in terms of QR, such as the fact that it is inconsistent with the copy theory of movement, which reintroduces the problem, and some problems such as late Merge (which is incompatible with Chomsky's claim that adjunction is cyclic), covert QR to the right, pertaining to Fox's recent work on ACD. The gist of Chomsky's (2004) proposal for ACD goes as follows:

(i) a. John likes every boy Mary does.
b. John likes every boy (that is, more accurately ...) every boy Mary likes.

The sentence in (ib) is taken by Chomsky (2004) to be the underlying structure of (ia). Here the second instance of *every boy* in (ib) is destressed in the adjoined phrase (i.e., *every boy Mary likes*) and can undergo ellipsis in the normal way, yielding (ia). If ACD is resolved in the manner suggested, the number of problems pointed out above will largely disappear, and ACD itself will also disappear as a phenomenon. It may be interesting to note that an ACD construction with an argument as head of the restrictive relative construction is treated here in terms of adjunction (which, according to Uriagereka and Pietroski 2002, may have the potential to grow in an unbound, infinite fashion), though it is not clear here whether infinite regress is at stake. Recall Uriagereka and Pietroski's (2002) results to the effect that arguments (as heads of a non-restrictive relative construction) disallow infinite regress, which is only possible with adjuncts, more specifically with disjuncts.

References

Adger, D., C. de Cat, and G. Tsoulas, eds. 2004. *Peripheries: Syntactic edges and their effects.* Dordrecht: Kluwer Academic Publishers.

Agamben, G. 2002/2004. *L'aperto: L'uomo e l'animale* (*The opened: The man and the animal/ Hirakare: Ningen to doubutsu*, translated into the Japanese by A. Okada and K. Taga). Torino: Bollati Boringhieri (Tokyo: Heibonsha).

Agbayani, B. 2006. Pied-piping, feature movement, and *wh*-subjects. In Wh-*movement: Moving on*, ed. by L.L.-S. Cheng and N. Corver, 71-93. Cambridge, Mass.: MIT Press.

Alexiadou, A. and E. Anagnostopoulou. 1998. Parametrizing AGR: Word order, V-movement, and EPP-checking. *Natural Language & Linguistic Theory* 16: 491-539.

Alexiadou, A. and E. Anagnostopoulou. 2001. The subject-in-situ generalization and the role of Case in driving computations. *Linguistic Inquiry* 32: 193-231.

Antony, L.M. and N. Hornstein, eds. 2003. *Chomsky and his critics.* Oxford: Blackwell.

Arima, M. 2006. Gengo-to Peirce-no kigouron (Language and Peircian semiotics). *Dai-Koukai: Voyages into History, Literature and Thought* 60: 90-99.

Avrutin, S. 2004. Beyond narrow syntax. In *Variation and universals in biolinguistics*, ed. by L. Jenkins, 95-115. Amsterdam: Elsevier.

Avrutin, S. 2006. Weak syntax. In *Broca's region*, ed. by Y. Grodzinsky and K. Amunts, 49-62. Oxford: Oxford University Press.

Baker, M.C. 2005. The innate endowment for language: Unspecified or overspecified? In *The innate mind: Structure and contents*, ed. by P. Carruthers, S. Laurence, and S. Stich, 156-174. Oxford: Oxford University Press.

Barwise, J. and R. Cooper. 1981/2002. Generalized quantifiers and natural language. In *Formal semantics: The essential readings*, ed. by P. Portner and B.H. Partee, 75-126. Oxford: Blackwell Publishing.

Belletti, A., ed. 2004. *Structures and beyond: The cartography of syntactic structures, volume 3.* Oxford: Oxford University Press.

Bertolo, S. 2001. *Language acquisition and learnability.* Cambridge: Cambridge University Press.

Bickerton, D. 1999. How to acquire language without positive evidence: What acquisitionists can learn from creoles. In *Language creation and language change*, ed. by M. DeGraff, 49-74. Cambridge, Mass.: MIT Press.

Bloom, P. 2000. *How children learn the meanings of words.* Cambridge, Mass.: MIT Press.

Boeckx, C. 2006. *Linguistic minimalism: Origins, concepts, methods, and aims.* Oxford: Oxford University Press.

Bošković, Ž. 2001. *On the nature of the syntax-phonology interface: Cliticization and related phenomena.* Amsterdam: Elsevier.

Bošković, Ž. and S. Franks. 2000. Across-the-board movement and LF. *Syntax* 3: 107-128.

Bošković, Ž. and H. Lasnik, eds. 2007. *Minimalist syntax: The essential readings.* Oxford: Blackwell.

Breheny, R., N. Katsos, and J. Williams. 2006. Are generalized scalar implicatures generated by default? An on-line investigation into the role of context in generating pragmatic inferences. *Cognition* 100: 434-463.

Breitbarth, A. and H. van Riemsdijk, eds. 2004. *Triggers.* Berlin: Mouton de Gruyter.

Brody, M. 2006. 'Move' in syntax: Logically necessary or undefinable in the best case? *Lingua* 116: 1553-1560.

Brooks, P. J. and I. Sekerina. Shortcuts to quantifier interpretation in children and adults. *Language Acquisition* 13: 177-206.

Brown, R. 1973. *A first language: The early stages.* Harmondsworth, Middlesex: Penguin Books.

Burzio, L. 1986. *Italian syntax.* Dordrecht: Reidel.

Carruthers, P. 2005. Distinctively human thinking: Modular precursors and components. In *The innate mind: Structure and contents*, ed. by P. Carruthers, S. Laurence, and S. Stich, 69-88. Oxford: Oxford University Press.

Cecchetto, C. 2004. Explaining the locality conditions of QR: Consequences for the theory of phases. *Natural Language Semantics* 12: 345-397.

Cheng, L.L.-S., N. Corver, eds. 2006. Wh-*movement: Moving on.* (Foreword by Noam Chomsky.) Cambridge, Mass.: MIT Press.

Chierchia, G. 2006. Broaden your views: Implicatures domain widening and the "logicality" of language. *Linguistic Inquiry* 37: 535-590.

Chierchia, G., S. Crain, M.T. Guasti, A. Gualmini, and L. Meroni. 2001. The acquisition of disjunction: Evidence for a grammatical view of scalar implicatures. In *BUCLD 25 Proceedings*, ed. by A.H.-J. Do et al., 157-168. Somerville, Mass.: Cascadilla Press.

Chomsky, N. 1965. *Aspects of the theory of syntax.* Cambridge, Mass.: MIT Press.

Chomsky, N. 1972a. *Studies on semantics in generative grammar.* The Hague: Mouton.

Chomsky, N. 1972b. Deep structure, surface structure and semantic interpretation. In *Studies on semantics in generative grammar*, 62-119. The Hague: Mouton.

Chomsky, N. 1975a. *Reflections on language.* New York: Pantheon Press.

Chomsky, N. 1975b. *The logical structure of linguistic theory.* New York: Plenum Press.

Chomsky, N. 1977a. *Essays on form and interpretation.* New York: North-Holland.

Chomsky, N. 1977b. On wh-movement. In *Formal syntax*, ed. by P.W. Culicover, T. Wasow, and A.

Akmajian, 71-132. New York: Academic Press.
Chomsky, N. 1981. *Lectures on government and binding.* Dordrecht: Foris.
Chomsky, N. 1995a. Bare phrase structure. In *Government and binding theory and the Minimalist Program*, ed. by G. Webelhuth, 383-439. Oxford: Blackwell.
Chomsky, N. 1995b. *The minimalist program.* Cambridge, Mass.: MIT Press.
Chomsky, N. 2000a. Minimalist inquiries: The framework. In *Step by step: Essays on minimalist syntax in honor of Howard Lasnik*, ed. by R. Martin, D. Michaels, and J. Uriagereka, 89-155. Cambridge, Mass.: MIT Press.
Chomsky, N. 2000b. *New horizons in the study of language and mind*, with Foreword by N. Smith. Cambridge: Cambridge University Press.
Chomsky, N. 2001. Derivation by phase. In *Ken Hale: A life in language*, ed. by Michael Kenstowicz, 1-52. Cambridge, Mass.: MIT Press.
Chomsky, N. 2002. *On nature and language*, ed. by A. Belletti and L. Rizzi. Cambridge: Cambridge University Press.
Chomsky, N. 2004. Beyond explanatory adequacy. In *Structures and beyond: The cartography of syntactic structures, Volume 3*, ed. by A. Belletti, 104-131. Oxford: Oxford University Press.
Chomsky, N. 2005a. Three factors in language design. *Linguistic Inquiry* 36: 1-22.
Chomsky, N. 2005b. On phases. Ms., MIT, Cambridge, Mass.
Chomsky, N. 2006. Approaching UG from below. Ms., MIT, Cambridge, Mass.
Chomsky, N. and M. Halle. 1968. *The sound pattern of English.* New York: Harper and Row.
Cinque, G. 1999. *Adverbs and functional heads.* Oxford: Oxford University Press.
Cinque, G., ed. 2002. *Functional structure in DP and IP: The cartography of syntactic structures, volume 1.* Oxford: Oxford University Press.
Citko, B. 2006. The interaction between across-the-board *wh*-movement and left-branch extraction. *Syntax* 9: 225-247.
Collins, C. 2002. Eliminating labels. In *Derivation and explanation in the Minimalist Program*, ed. by S.D. Epstein and T.D. Seely, 42- 64. Oxford: Blackwell.
Cook, V.J. and M. Newson. 1996. *Chomsky's universal grammar: An introduction (second edition).* Oxford: Blackwell.
Craenenbroeck, J. van and M. den Dikken. 2006. Ellipsis and EPP repair. *Linguistic Inquiry* 37: 653-664.
Crain, S., A. Gualmini, and P. Pietroski. 2005. Brass tacks in linguistic theory: Innate grammatical principles. In *The innate mind: Structure and contents*, ed. by P. Carruthers, S. Laurence, and S. Stich, 175-197. Oxford: Oxford University Press.
Crain, S. and D. Lillo-Martin. 1999. *An introduction to linguistic theory and language acquisition.* Oxford: Blackwell.
Crain, S. and P. Pietroski. 2002. Why language acquisition is a snap. *The Linguistic Review* 19: 163-183.
Crain, S. and R. Thornton. 1998. *Investigations in universal grammar: A guide to experiments on the acquisition of syntax and semantics.* Cambridge, Mass.: MIT Press.
Danon-Boileau, L. 2002/2006. *Children without language: From dysphasia to autism* (translated from the French by J. Grieve). Oxford: Oxford University Press.
Davidson, D. 1967. The logical form of action sentences. In D. Davidson 1980.
Davidson, D. 1980. *Essays on actions and events.* Oxford: Oxford University Press.
Deleuze, G. and F. Guattari. 1972//1983/2004. *Anti-Oedipus: Capitalism and schizophrenia* (translated from the French by R. Hurley, M. Seem, and H.R. Lane; *Preface* by M. Foucault). London: Continuum.
Donati, C. 2006. On *wh*-head movement. In Wh-*movement: Moving on*, ed. by L.L.-S. Cheng and N. Corver, 21-46. Cambridge, Mass.: MIT Press.

Dresher, B.E. 1999. Charting the learning path: Cues to parameter setting. *Linguistic Inquiry* 30: 27-67.

Dresher, B.E. and J.D. Kaye. 1990. A computational learning model for metrical phonology. *Cognition* 34: 137-195.

Edwards, S. 2005. *Fluent aphasia.* Cambridge: Cambridge University Press.

Emmorey, K. 2006. The role of Broca's area in sign language. In *Broca's region*, ed. by Y. Grodzinsky and K. Amunts, 169-184. Oxford: Oxford University Press.

Endou, H. 2006. Process-to-shiteno kokoro (The mind as a process). *Dai-Koukai: Voyages into History, Literature and Thought* 60: 115-121.

Epstein, S.D., E.M. Groat, R. Kawashima, and H. Kitahara. 1998. *A derivational approach to syntactic relations.* Oxford: Oxford University Press.

Epstein, S.D. and T.D. Seely. 1999. SPEC-ifying the GF "subject;" eliminating A-chains and the EPP within a derivational model. Draft for *Grammatical Functions Workshop, LSA Institute 1999.*

Epstein, S.D. and T.D. Seely. 2006. *Derivations in minimalism.* Cambridge: Cambridge University Press.

Fitch W.T., M.D. Hauser, and N. Chomsky. 2005. The evolution of the language faculty: Clarifications and implications. *Cognition* 97: 179-210.

Fodor, J.A. 1970. Three reasons for not deriving *kill* from *cause to die. Linguistic Inquiry* 1: 429-438.

Fodor, J.A. 1983. *The modularity of mind.* Cambridge, Mass.: MIT Press.

Fodor, J.A. 2000. *The mind doesn't work that way.* Cambridge, Mass.: MIT Press.

Fodor, J.D. 1998. Unambiguous triggers. *Linguistic Inquiry* 29: 1-36.

Fodor, J.D. and C. Crowther. 2002. Understanding stimulus poverty arguments. *The Linguistic Review* 19: 105-145.

Fox, D. 1995. Condition C effects in ACD. In *MIT working papers in linguistics 27*, ed. by R. Pensalfini and H. Ura, 105-120. Cambridge, Mass.: MITWPL.

Fox, D. 2000. *Economy and semantic interpretation.* Cambridge, Mass.: MIT Press.

Fox, D. and D. Pesetsky. 2005. Cyclic linearization of syntactic structure. *Theoretical Linguistics* 31: 1-45.

Frampton, J. and S. Gutmann. 1999. Morphological well-formedness as a derivational constraint on syntax: Verbal inflection in English. Ms., Northeastern University.

Frampton, J. and S. Gutmann. 2002. Crash-proof syntax. In *Derivation and explanation in the Minimalist Program*, ed. by S.D. Epstein and T.D. Seely, 90-105. Oxford: Blackwell.

Frampton, J. and S. Gutmann. 2006. How sentences grow in the mind: Agreement and selection in an efficient minimalist syntax. In *Agreement systems*, ed. by C. Boeckx, 121-157. Amsterdam: John Benjamins.

Franck, J., G. Lassi, U.H. Frauenfelder, and L. Rizzi. 2006. Agreement and movement: A syntactic analysis of attraction. *Cognition* 101: 173-216.

Friederici, A.D. 2006. The neural basis of sentence processing: Inferior frontal and temporal contributions. In *Broca's region*, ed. by Y. Grodzinsky and K. Amunts, 196-217. Oxford: Oxford University Press.

Froud, K. 2006. Unaccusativity as lexical argument reduction: Evidence from aphasia. *Cognition* 116: 1631-1650.

Fujimoto, T. 2006. Uchuu-shinkaron-to kangen-shugi-no kouzai (The evolutionary theory of the universe and the pros and cons of reductionism). *Dai:Koukai: Voyages into History, Literature and Thought* 60: 32-38.

Fujita, K. 2005. Gengo-o gengo tarashimeru mono (What makes language language). *Gengo* 34: 30-37.

Fukui, N. 2006. *Theoretical comparative syntax: Studies in macroparameters.* London: Routledge.

Friedmann, N. 2006. Speech production in Broca's agrammatic aphasia: Syntactic tree pruning. In *Broca's region*, ed. by Y. Grodzinsky and K. Amunts, 63-82. Oxford: Oxford University Press.

Gelman, S.A. 2005. Two insights about naming in the preschool child. In *The innate mind: Structure and contents*, ed. by P. Carruthers, S. Laurence, and S. Stich, 198-215. Oxford: Oxford University Press.

Gill, J.H. 1997. *If a chimpanzee could talk: and other reflections on language acquisition.* Tucson: The University of Arizona Press.

Gill, K.-H. and G. Tsoulas. 2004. Peripheral effects without peripheral syntax: The left periphery in Korean. In *Peripheries: Syntactic edges and their effects*, ed. by D. Adger, C. de Cat, and G. Tsoulas, 121-141. Dordrecht: Kluwer.

Goodall, G. 1987. *Parallel structures in syntax: Coordination, causatives, and restructuring.* Cambridge: Cambridge University Press.

Gopnik, A. 2003. The theory theory as an alternative to the innateness hypothesis. In *Chomsky and his critics*, ed. by L.M. Antony and N. Hornstein, 238-254. Oxford: Blackwell.

Grice, P. 1989. *Studies in the way of words.* Cambridge, Mass.: Harvard University Press.

Grodzinsky, Y. 2006. A blueprint for a brain map of syntax. In *Broca's region*, ed. by Y. Grodzinsky and K. Amunts, 83-107. Oxford: Oxford University Press.

Grodzinsky, Y. and T. Reinhart. 1993. The innateness of binding and coreference. *Linguistic Inquiry* 24: 69-101.

Gualmini, A. and S. Crain. 2002. Why no child or adult must learn De Morgan's laws. In *BUCLD 26 Proceedings*, 243-254. Somerville, Mass.: Cascadilla Press.

Gualmini, A. and S. Crain. 2004. Operator conditioning. Ms., MIT and the University of Maryland at College Park. (To appear in *BULCD 28 Proceedings*, Somerville, Mass.: Cascadilla Press.)

Gualmini, A., S. Crain, L. Meroni, G. Chierchia, and M.T. Guasti. 2001. At the semantics/pragmatics interface in child language. In *Proceedings of SALT XI*, 231-247. Ithaca, NY: Cornell University.

Guasti, M.T. 2002. *Language acquisition: The growth of grammar.* Cambridge, Mass.: MIT Press.

Halle, M., J. Bresnan, and G.A. Miller, eds. 1978. *Linguistic theory and psychological reality.* Cambridge, Mass.: MIT Press.

Harman, G., ed. 1974. *On Noam Chomsky: Critical essays.* New York: Anchor Press.

Hauser, M.D., N. Chomsky, and T. Fitch. 2002. The faculty of language: What is it, who has it, and how did it evolve? *Science* 298: 1569-1579.

Heim, I. 1983/2002. File change semantics and the familiarity theory of definiteness. In *Formal semantics: The essential readings*, ed. by P. Portner and B.H. Partee, 223-248. Oxford: Blackwell.

Helen Keller Campaigning Committee/*The Mainichi. 1948/2003. Helen Keller monogatari (The story of Helen Keller).* At http://www.helenkeller.or.jp/hkstory.htm

Hinzen, W. 2006. *Mind design and minimal syntax.* Oxford: Oxford University Press.

Hornstein, N. and D. Lightfoot. 1981. *Explanation in linguistics: The logical problem of language acquisition.* London: Longman.

Horwich, P. 2003. Meaning and its place in the language faculty. In *Chomsky and his critics*, ed. by L.M. Antony and N. Hornstein, 162-178. Oxford: Blackwell.

Imafuku, R. 1989/2001. *Kouya-no Romanesque (Romanesque in the wilds).* Tokyo: Iwanami shoten.

Iwata, R. 1998. *Sanji-gen bunpou-ron (A three-dimensional theory of grammar).* Tokyo: Eichosha.

Jackendoff, R. 1972. *Semantic interpretation in generative grammar.* Cambridge, Mass.: MIT Press.

Jenkins, L. 2000. *Biolinguistics: Exploring the biology of language.* Cambridge: Cambridge University Press.

Johnson, D.E. and S. Lappin. 1999. *Local constraints vs. economy.* Stanford, Calif.: CSLI Publications.

Kaku, M. 2005/2006. *Parareru waarudo (Parallel worlds: A journey through creation, higher dimensions, and the future of the cosmos*, translated into the Japanese by T. Saito). New York: Stuart Krichevsky Literary Agency, Inc. (Tokyo: Nihon-Housou Shuppan Kyoukai).

Kasai, K. 2006. *Tantei-shousetsu to kigou-teki ningen: Minerva-no fukurou-wa tasogare-ni tobitatsuka? (Detective stories and the problem of "character": Does Minerva's owl fly off at*

twilight?). Tokyo: Tokyo Soogensha.
Kayne, R.S. 1984. *Connectedness and binary branching.* Dordrect: Foris.
Kayne, R.S. 1994. *The antisymmetry of syntax.* Cambridge, Mass.: MIT Press.
Kayne, R.S. 2000. *Parameters and universals.* Oxford: Oxford University Press.
Kayne, R.S. 2004. Antisymmetry and Japanese. In *Variation and universals in biolnguistics*, ed. by L. Jenkins, 3-35. Amsterdam: Elsevier.
Kayne, R.S. 2005. *Movement and silence.* Oxford: Oxford University Press.
Kegl, J. 2004. Language emergence in a language-ready brain: Acquisition. In *Variation and universals in biolinguistics*, ed. by L. Jenkins, 195-236. Amsterdam: Elsevier.
Kiguchi, H. and R. Thornton. 2004. Binding principles and ACD constructions in child grammars. *Syntax* 7: 234-271.
Komori, K. 2003. *G-no zan-ei (The residue of G): Pyotor Demianovitch Ouspensky unremembered.* Tokyo: Bungei-shunjuu.
Koppen, M. van. 2006. One probe — multiple goals. *Glow Newsletter* 56.
Lasnik, H. 2005. Grammar, levels, and biology. In *The Cambridge companion to Chomsky*, ed. by J. McGilvray, 60-83. Cambridge: Cambridge University Press.
Legate, J.A. and C.D. Yang. 2002. Empirical re-assessment of stimulus poverty arguments. *The Linguistic Review* 19: 151-162.
Lem, S. 1968/1976//2005. *Głos pana/Katar* (*The voice from Heaven/Hay fever//Ten-no koe/ Karekusa-netsu*, translated into the Japanese by M. Numano, D. Fukami, and S. Yoshigami). Tokyo: Kokusho-Kankoukai.
Lidz, J. and J. Musolino. 2005/2006. On the quantificational status of indefinites: The view from child language. *Language Acquisition* 13: 73-102.
Lieberman, P. 1984, 2006. *Toward an evolutionary biology of language.* Cambridge, Mass.: Harvard University Press.
Lightfoot, D.W. 1994. Degree-0 learnability. In *Syntactic theory and first language acquisition: Cross-linguistic perspectives, volume 2: Binding, dependencies, and learnability*, ed. by B. Lust, G. Hermon, and J. Kornfilt, 453-471. Hillsdale, N.J.: Lawrence Erlbaum.
Lightfoot, D.W. 1999. *The development of language: Acquisition, change, and evolution.* Oxford: Blackwell.
Lightfoot, D.W. 2006. *How new languages emerge.* Cambridge: Cambridge University Press.
Longobardi, G. 1994. Reference and proper names. *Linguistic Inquiry* 25: 609-665.
López, L. 2003. Steps for a well-adjusted dislocation. *Studia Linguistica* 57: 193-231.
Ludlow, P. 2003. Referential semantics for I-languages? In *Chomsky and his critics*, ed. by L.M. Antony and N. Hornstein, 140-161. Oxford: Blackwell.
Maeno, T. 2006. Jiyuu-ishi-wa sonzai-shinai-ka (Does free will not exist?). *Gendai-Shisoo (Revue de la Pensée d'Aujourd'hui)* 34-11: 96-101.
Maher, J. and J. Groves. 1996. *Chomsky for beginners.* Cambridge: Icon Books.
Marušić, F. 2006. If non-simultaneous spell-out exists, this is what it can explain. *Glow Newsletter* 56.
May, R. 1985. *Logical form: Its structure and derivation.* Cambridge, Mass.: MIT Press.
McGilvray, J. 2005. *The Cambridge companion to Chomsky.* Cambridge: Cambridge University Press.
Merchant, J. 2001. *The syntax of silence: Sluicing, islands, and identity in ellipsis.* Oxford: Oxford University Press.
Mithen, S. 2005/2006. *The singing Neanderthals: The origins of music, language, mind and body* (*Utau Neanderutaaru: Ongaku to gengo-kara miru hito-no shinka*, translated into the Japanese by J. Kumagai). London: Weindenfeld & Nicolson Ltd (Tokyo: Hayakawa Publishing, Inc.).
Miyagawa, S. 2006. On the "undoing" property of scrambling: A response to Bošković. *Linguistic Inquiry* 37: 607-624.
Moro, A. 1997. *The raising of predicates: Predicative noun phrases and the theory of clause structure.*

Cambridge: Cambridge University Press.
Moro, A. 2000. *Dynamic antisymmetry.* Cambridge, Mass.: MIT Press.
Moro, A. 2004. Linear compression as a trigger for movement. In *Triggers*, ed. by A. Breitbarth and H. van Riemsdijk, 387-429. Berlin: Mouton de Gruyter.
Mulders, I. 2005. Transparent parsing: Phases in sentence processing. In *MIT working papers in linguistics, volume 49: Perspectives on phases*, ed. by M. McGinnis and N. Richards, 237-264. Cambridge, Mass.: MITWPL.
Musolino, J., S. Crain, and R. Thornton. 2000. Navigating negative quantificational space. *Linguistics* 38: 1-32.
Myouki, H. 2006. Lacan-to iu na (The name of *Lacan*). *Dai-Koukai: Voyages into History, Literature and Thought*, 59:142-150.
Nakazawa, S. 2002. *Kuma-kara Ou-e (From the bear to the king): Cahier sauvage II.* Tokyo: Kodansha.
Nakazawa, S. 2004. *Taishousei jinruigaku (Symmetric anthropology): Cahier sauvage V.* Tokyo: Kodansha.
Nietzsche, F. 1889, 1895/1968/1990/2003. *Twilight of the idols and The anti-Christ*, translated into the English by R.J. Hollingdale (with the 1990 Introduction by M. Tanner). London: Penguin Books.
Niyogi, P. 2006. *The computational nature of language learning and evolution.* Cambridge, Mass.: MIT Press.
Norris, C. 2004. *Language, logic and epistemology: A modal-realist approach.* New York: Palgrave Macmillan.
Nowak, M.A., N.L. Komarova, and P. Niyogi. 2001. Evolution of universal grammar. *Science* 291: 114-118. .
Nunes, J. 2004. *Linearization of chains and sideward movement.* Cambridge, Mass.: MIT Press.
Oota, T. 2006. Hyoushou-no mikuro-bunsekigaku: Gnosis-kara Lacan-e (A micro-analytical theory of the symbol: From gnosis to Lacan). *Dai-Koukai: Voyages into History, Literature and Thought* 59: 151-157.
Pearl, L. 2003/2004. Addressing acquisition from language change: A modeling perspective. Ms., University of Maryland at College Park.
Peirce, C.S. 1887/1992/2006. A guess at the riddle (Nazo-eno suiryou, translated into the Japanese by N. Mitani). In *The essential Peirce: Selected philosophical writing 1*, ed. by The Peirce Edition Project, Indiana University Press, 1992, 245-279 (*Dai-Koukai: Voyages into History, Literature and Thought* 60: 140-152.
Perovic, A. 2006. Syntactic deficit in Down syndrome: More evidence for the modular organization of language. *Cognition* 116: 1616-1630.
Petitto, L.-A. 2005. How the brain begets language. In *The Cambridge companion to Chomsky*, ed. by J. McGilvray, 84-101. Cambridge: Cambridge University Press.
Phillips, C. 1996. Order and structure. Doctoral dissertation, MIT, Cambridge, Mass.
Phillips, C. 2003. Linear order and constituency. *Linguistic Inquiry* 34: 37-90.
Piattelli-Palmarini, M. and J. Uriagereka. 2004. The immune syntax: The evolution of the language virus. In *Variation and universals in biolinguistics*, ed. by L. Jenkins, 341-377. Amsterdam: Elsevier.
Pietroski, P. 2003. Small verbs, complex events: Analyticity without synonymy. In *Chomsky and his critics*, ed. by L.M. Antony and N. Hornstein, 000-000. Oxford: Blackwell.
Pietroski, P. 2005. *Events and semantic architecture.* Oxford: Oxford University Press.
Pinker, S. 1984. *Language learnability and language development.* Cambridge, Mass.: Harvard University Press.
Pinker, S. 1994. How could a child use verb syntax to learn verb semantics? *Lingua* 92: 377-410.
Pustejovsky, J. 1995. *The generative lexicon.* Cambridge, Mass.: MIT Press.
Reinhart, T. 1991. Elliptic conjunctions — non-quantificational LF. In *The Chomskyan turn*, ed. by A.

Kasher, 360-384. Cambridge, Mass.: Basil Blackwell.
Reinhart, T. 1997. Quantifier scope: How labor is divided between QR and choice functions. *Linguistics and Philosophy*, 20: 335-397.
Reinhart, T. 2004. The processing cost of reference set computation: Acquisition of stress shift and focus. *Language Acquisition* 12: 109-155.
Reinhart, T. 2006. *Interface strategies: Optimal and costly computations.* Cambridge, Mass.: MIT Press.
Reinhart, T. and E. Reuland. 1993. Reflexivity. *Linguistic Inquiry* 24: 657-720.
Reuland, E. 2001. Primitives of binding. *Linguistic Inquiry* 32: 439-492.
Rey, G. 2003. Chomsky, intentionality, and a CRTT. In *Chomsky and his critics*, ed. by L.M. Antony and N. Hornstein, 105-139. Oxford: Blackwell.
Rezac, M. 2006. The interaction of Th/Ex and locative inversion. *Linguistic Inquiry* 37: 685-697.
Rice, M.L., ed. 1996. *Toward a genetics of language.* Mahwah, NJ: Lawrence Erlbaum Associates, Publishers.
Richards, N. 2001. *Movement in language.* Oxford: Oxford University Press.
Riemsdijk, H. van. 2006. Grafts follow from merge. In *Phases of interpretation*, ed. by M. Frascarelli, 17-44. Berlin: Mouton de Gruyter.
Rizzi, L. 1997. The fine structure of the left periphery. In *Elements of grammar: Handbook of generative syntax*, ed. by L. Haegeman, 281-337. Dordrecht: Kluwer.
Rizzi, L., ed. 2004. *The structure of CP and IP: The cartography of syntactic structures, volume 2.* Oxford: Oxford University Press.
Roca, I.M., ed. 1990. *Logical issues in language acquisition.* Dordrecht: Foris Publications.
Rooryck, J. 2006. Binding into pronouns. *Lingua* 116: 1561-1579.
Ruthrof, H. 1997. *Semantics and the body: Meaning from Frege to the postmodern.* Toronto: University of Toronto Press.
Sakai, T. 2006. Tasha-no tobari-ga yaburareru-toki : Bataille to Lacan (When the partition for others is destroyed: Bataille and Lacan). *Dai-Koukai: Voyages into History, Literature and Thought* 59: 166-175.
Sakas, W.G. and J.D. Fodor. 2001. The structural triggers learner. In *Language acquisition and learnability*, ed. by S. Bertolo, 172-233. Cambridge: Cambridge University Press.
Sandler, W. and D. Lillo-Martin. 2006. *Sign language and linguistic universals.* Cambridge: Cambridge University Press.
Schwabe, K. and S. Winkler, eds. 2003. *The interfaces: Deriving and interpreting omitted structures.* Amsterdam: John Benjamins.
Shinguu, K. and M. Miura (an interview). 2006. Lacan-e mukau michi (A way leading to Lacan). *Dai-Koukai: Voyages into History, Literature and Thought*, 59: 42-71.
Sigurðsson, H.Á. 1991. Icelandic Case-marked PRO and the licensing of lexical arguments. *Natural Language & Linguistic Theory* 9: 327-363.
Sigurðsson, H.Á. 2006a. The nominative puzzle and the low nominative hypothesis. *Linguistic Inquiry* 37: 289-308.
Sigurðsson, H.Á. 2006b. Agree in syntax, agreement in signs. In *Agreement systems*, ed. by C. Boeckx, 201-237. Amsterdam: John Benjamins.
Simpson, T., P. Carruthers, S. Laurence, and S. Stich. Introduction: Nativism past and present. In *The innate mind: Structure and contents*, ed. by P. Carruthers, S. Laurence, and S. Stich, 3-19. Oxford: Oxford University Press.
Stewart, I. 1998. *Life's other secret: The new mathematics of the living world.* New York: John Wiley & Sons, Inc.
Suzuki, N. 1997. *Seisei-tougo-ron kenyuu (Studies in generative syntax).* Tokyo: Eichoosha.
Suzuki, N. 2001. Minimalism and its implications for language acquisition and learnability. In *Kobe*

Shinwa studies in English linguistics and literature 21, 14-43. Kobe: Society of English Linguistics and Literature, Kobe Shinwa Women's University. Also available at http://ci.nii.ac.jp/vol_issue/nels/AN10470675_en.html

Suzuki, N. 2002. Triggers, minimalism, and learnability. In *Kobe Shinwa studies in English linguistics and literature* 22, 1-29. Kobe: Society of English Linguistics and Literature, Kobe Shinwa Women's University. Also available at http://ci.nii.ac.jp/vol_issue/nels/AN10470675_en.html

Suzuki, N. 2003. Universals, Occam's razor, and the strong minimalist thesis. In *Kobe Shinwa studies in English linguistics and literature* 23, 1-17. Kobe: Society of English Linguistics and Literature, Kobe Shinwa Women's University. Also available at http://ci.nii.ac.jp/vol_issue/nels/AN10470675_en.html

Suzuki, N. 2005. How the reality can be compatible with the strong minimalist thesis: Interfacing, economy, and variation. In *Kobe Shinwa studies in English linguistics and literature* 24, 1-92. Kobe: Society of English Linguistics and Literature, Kobe Shinwa Women's University. Also available at http://ci.nii.ac.jp/vol_issue/nels/AN10470675_en.html

Suzuki, N. 2006. Two case studies in language acquisition: Evolution, efficient computation, and learnability. In *Kobe Shinwa studies in English linguistics and literature* 25, 1-81. Kobe: Society of English Linguistics and Literature, Kobe Shinwa Women's University.

Takahashi, S. 1999. *Gödel-no tetsugaku: Fukanzensei-teiri to kami-no sonzairon (Gödel's philosophy: Incompleteness theorems and the ontology of God).* Tokyo: Kodansha.

Terada, H. 1999. Successive cyclicity and incrementality. *English Linguistics* 16: 243-274.

Terada H. 2002. On θ-checking. *English Linguistics* 19: 1-28.

Tesan, G. 2003/2004. The inflectional category in child English: A parametric account. Ms., University of Maryland at College Park.

Tesan, G. and R. Thornton. 2003. Emergence of *do*-support in child English. Ms., University of Maryland at College Park.

Tesan, G. and R. Thornton. 2003/2004. Overspecification in early English. Ms., University of Maryland at College Park. (To appear in the *Proceedings of the 10th GALA Conference*, UiL OTS/ Utrecht University, The Netherlands.)

Thompson, E. and F.J. Varela. 2001/2006. Radikaru-na shintai-ka: Shinkei-danamikusu to ishiki (translated from the English by K. Takahata; Radical embodiment: Neural dynamics and consciousness). *Gendai-shisoo (Revue de la Pensée d'Aujourd'hui)* 34-11: 82-95 (*Trends in Cognitive Science* 5: 418-425).

Unami, A. 2006a. Signifiant-ni hikari are (Let there be light for the *signifiant*). *Dai-Koukai: Voyages into History, Literature and Thought* 59: 80-87.

Unami, A. 2006b. Abduction-no senkou (The flash of abduction). *Dai-Koukai: Voyages into History, Literature and Thought* 60: 100-107.

Uriagereka, J. 1999. Warps: Some thoughts on categorization. *Theoretical Linguistics* 25: 31-73. (Reprinted in Uriagereka 2002: Chapter 15.)

Uriagereka, J. 2002. *Derivations: Exploring the dynamics of syntax.* London: Routledge.

Uriagereka, J. 2006. Complete and partial Infl. In *Agreement systems*, ed. by C. Boeckx, 267-298. Amsterdam: John Benjamins.

Uriagereka, J. and P. Pietroski. 2002. Dimensions of natural language. In *Derivations: Exploring the dynamics of syntax*, 266- 287. London: Routledge.

Watanabe, A. 2006. The pied-piper feature. In Wh-*movement: Moving on*, ed. by L.L.-S. Cheng and N. Corver, 47-70. Cambridge, Mass.: MIT Press.

Weissenborn, J., H. Goodluck, and T. Roeper, eds. 1992. *Theoretical issues in language acquisition: Continuity and change in development.* Hillsdale, NJ: Lawrence Erlbaum Associates, Publishers.

Weissenborn, J. and B. Höhle, eds. 2001a. *Approaches to bootstrapping: Phonological, lexical, syntactic and neurophysiological aspects of early language acquisition, Volume 1.* Amsterdam: John

Benjamins.
Weissenborn, J. and B. Höhle, eds. 2001b. *Approaches to bootstrapping: Phonological, lexical, syntactic and neurophysiological aspects of early language acquisition, Volume 2.* Amsterdam: John Benjamins.
Wexler, K. 1994. Optional infinitives, head movement and the economy of derivations. In *Verb movement*, ed. by D. Lightfoot and N. Hornstein. Cambridge: Cambridge University Press.
Wexler, K. 1996. The development of inflection in a biologically based theory of language acquisition. In *Toward a genetics of language*, ed. by M.L. Rice. Mahwah, N.J.: Lawrence Erlbaum Associates, Inc.
Wexler, K. 1998. Very early parameter setting and the unique checking constraint: A new explanation of the optional infinitive stage. *Lingua* 106: 23-79.
Wexler, K., C.T. Schütze, and M. Rice. 1998. Subject case in children with SLI and unaffected controls: Evidence for the Agr/Tns omission model. *Language Acquisition* 7: 317-344.
White, L. 2003. *Second language acquisition and universal grammar.* Cambridge: Cambridge University Press.
Williams, E. 1978. Across-the-board rule application. *Linguistic Inquiry* 9: 31-43.
Winkler, S. 2005. *Ellipsis and focus in generative grammar.* Berlin: Mouton de Gruyter.
Yang, C.D. 2002. *Knowledge and learning in natural language.* Oxford: Oxford University Press.
Yang, C.D. 2004. Toward a theory of language growth. In *Variation and universals in biolinguistics,* ed. by L. Jenkins, 37-56. Amsterdam: Elsevier.
Yuasa, H. 1997/2006. *Bataille: Shoujin (Bataille: Consumation/Consumption).* Tokyo: Kodansha.
Žižek, S. 2005. *Interrogating the real.* London: Continuum.